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Effects of Form and Rotation of the Earth upon Ranges of Projectiles

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THE elementary equation,

$$\text{Range} = V_0^2 \sin 2\alpha/g, \quad (1)$$

for the case of a projectile dispatched with initial speed V_0 and angle of elevation α, possesses a simplicity gained by the acceptance of a number of approximations. Since the approximations are in general good ones, Eq. (1) is almost right in many cases, but if V_0 exceeds 11,050 m/sec, it is not correct even as to order of magnitude. The equation is subject to at least the following criticisms. *It has been assumed in deriving Eq. (1):*

(*1*) *That* g, the acceleration due to gravity, is constant in magnitude along the trajectory, whereas it really varies with both altitude and latitude.

(*2*) *That* gravitation is the only force acting upon the projectile. Usually air resistance is also present.

(*3*) *That* the initial and end points of the trajectory are in a plane normal to the direction of g. The curvature of the surface of the earth makes this condition difficult to realize for some modern projectiles.

(*4*) *That* the gravitational force is uniform in direction at any instant throughout the region occupied by the trajectory. A better approximation would regard the plumb lines as converging toward a point at or near the earth's center.

(*5*) *That* this common direction and the orientation of the plane surface do not change with respect to Galilean space, a condition impossible of realization upon a rotating earth.

Assumption (*1*) tends to make the range of Eq. (1) too short, while those of (*2*) and (*4*) operate in the opposite sense. Assumption (*5*) has the interesting property that the magnitude and sign of its effect upon the predicted range depend upon the direction in which the projectile is launched. What next follows here is a discussion of the effects of removing the restrictions of assumption (*5*). We shall deduce correction terms to be added to the right-hand member of Eq. (1), to make it more applicable to the measured ranges of projectiles started from the surface of the rotating earth.

If excuses are needed for re-opening a problem once disposed of by d'Alembert, Gauss, Laplace and Poisson, they are to be found in the facts that the literature of the subject contains many errors, that the conclusions of the present paper are needed for one which is to follow, that popular interest in the subject has recently developed, and that, so far as the writer knows, there is no readily available discussion in English which deduces the correction terms in an elementary way and gives them visualizable physical significance.

The corrections called for by the listed assumptions are usually small enough so that they may be developed quite independently and added to the range of Eq. (1) as separate perturbations. Assumption (*5*), concerning the rotational motion of the earth, will be abandoned here. Our argu-

ment concerns a spherically symmetrical earth of radius R, rotating upon its north-south diameter with angular speed ω although not possessed of other motions (such as orbital motion about the sun) which might affect projectile motion, and not encumbered by a resisting atmosphere.

TRAJECTORIES IN THE EQUATORIAL PLANE

We consider first some experiments with projectiles moving in the plane of the equator. Let the earth's rotation be arrested, and let a missile be projected from the earth's surface at some point on the equator with initial horizontal and vertical velocity components v_H and v_V. The component v_V will here necessarily be directed upward; v_H will, until further notice, lie in the equatorial plane, the eastward direction being taken as positive. The resulting trajectory will be that labeled A in Fig. 1, and the range is given by

$$OD = 2v_V v_H / g_1. \qquad (2)$$

As will be recalled, this formula involves the assumptions that g_1 is constant in magnitude and direction throughout the region occupied by the trajectory A and that the point D is on the tangent to the equator drawn through the starting point (rather than on the equator itself). The quantity g_1 is the Newtonian component of g—the ordinary earth-observed gravitational acceleration, in which the centrifugal component has been suppressed by bringing the earth to rest. In general, $g = g_1 - R\omega^2 \cos^2 \lambda$, where λ is the geographic latitude.

With the earth's roation resumed, let the projectile experiment be repeated, the mass being dispatched from the moving surface at the moment when the thrower coincides with O. The point of view in Fig. 1 now is that of a detached observer not participating in the earth's motion of rotation, and the coordinate system yOx is fixed in his Galilean space. Evidently we are regarding the earth from some point south of the equator and are looking in a northward direction parallel to the earth's axis. With respect to the detached observer and his coordinate system the new trajectory, B, will differ from A only in the possession of a different horizontal, or x-directed, extension. The previous component v_V still obtains, but the previous horizontal com-

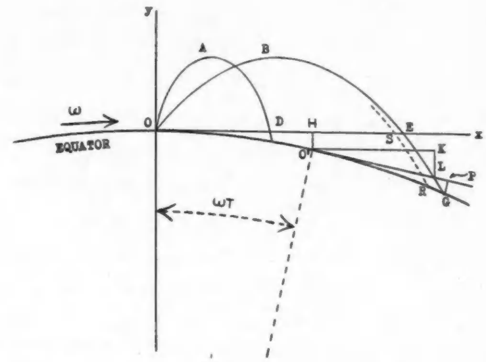

FIG. 1. Trajectories in the plane of the earth's equator as seen from the south. Trajectory A is obtained with the earth at rest and trajectory B, during rotation. Both curves are referred to the non-rotating coordinate system yOx.

ponent v_H has been augmented by the earth's peripheral speed and has the total value $v_H + R\omega$.

Still ignoring the increasingly serious fact that the gravitational field is convergent, we may denote the range by

$$OE = 2v_V(v_H + R\omega)/g_1. \qquad (3)$$

The appropriate acceleration is g_1 as before, since the earth's attraction is not affected by rotational motion. The duration of the flight T from O to E is $2v_V/g_1$.

DISPLACEMENT OF THE LANDING SURFACE

The flight is not terminated at E since the earth's surface has been displaced in the negative y direction by the curvature and rotation of the earth, enabling the projectile to achieve an additional range increment while continuing along EG. This means that the range as measured either by the detached or by the local observer differs in value from that predicted by his elementary formula [Eq. (3) or (2)]. The total range for the local observer will now be evaluated.

The procedure will be to determine the y component of the displacement from E to G; to obtain the additional duration of flight ΔT by dividing this displacement component by v_V, the y component of velocity; and to evaluate the total range by taking the product $v_H(T + \Delta T)$. This treatment is confined to trajectories for which $\omega T \ll 1 \gg v_H T/R$, so nothing will be done about the fact that the local observer's measured

range is not strictly parallel to the x axis. With like justification the y displacement along EG will be evaluated by adding scalarly three segments that are not strictly collinear but inclined at angles of the order of ωT, or $v_H T/R$. These approximations leave the argument still applicable with high accuracy to most projectiles and certainly to all those exempt from the necessity of large corrections for air resistance.

While the projectile has travelled from O to E in Fig. 1, its point of departure on the earth's surface has advanced from O to O'. A straight line drawn parallel to the y axis through O' intersects the x axis at H. A tangent to the equator through O' cuts the trajectory at L. A straight line drawn parallel to the y axis through L intersects at K a straight line parallel to the x axis through O'. A line through G perpendicular to the tangent $O'L$ meets $O'L$ at P.

The required y component of the displacement EG is approximately the sum $HO'+KL+PG$. For the case shown in Fig. 1, the absolute values of these segments are to be added; but for westward projection (v_H negative), G lies to the left of O' and the absolute value of KL should be subtracted from the sum of the other two. However, this point does not need to be remembered as the analysis to follow takes care of it automatically. To an excellent approximation (sagitta formula),

$$HO' = (OH)^2/2R = \tfrac{1}{2}R\omega^2T^2 = 2R\omega^2 v_V^2/g_1^2.$$

Also,

$$KL = O'K \tan \omega T = OD\omega T = 4\omega v_V^2 v_H/g_1^2$$

and

$$PG = (O'P)^2/2R = (OD)^2/2R = 2v_V^2 v_H^2/Rg_1^2.$$

Collecting these expressions we have:

Local range corrected for displaced landing surface $= v_H(T+\Delta T)$

$$= v_H\left(\frac{2v_V}{g_1}+\frac{HO'+KL+PG}{v_V}\right)$$

$$= \frac{2v_V v_H}{g_1}+\frac{2v_V v_H}{Rg_1^2}[(R\omega)^2+2R\omega v_H+v_H^2]. \quad (4)$$

TIME VARIATION OF DIRECTION OF GRAVITY

It must now be recalled that the projectile would land at G only if the gravitational force were at all times parallel to the y axis. Such is indeed its direction at the start of the flight, but a backward (negative x) component of force and consequent acceleration sets in as soon as the projectile leaves O and increases steadily as the body progresses in the positive x direction. The effect of this backward pull is to shorten the range so that the path terminates at a point R in Fig. 1. Equation (4) gives a range that is too great by the amount RG, an increment which we proceed to evaluate. The dotted line SR shows a portion of the corrected trajectory.

The acceleration of the projectile at any instant of its flight has the magnitude g_1 and is directed toward the center of the earth. The x component of this acceleration increases in absolute value continuously during the flight, having at any time t the value $-g_1 \sin(\omega+v_H/R)t$, where t is the time measured from the start of the flight. For ordinary projectiles there is small loss of rigor in approximating the sine and writing

$$\frac{d^2x}{dt^2} = -g_1\left(\omega+\frac{v_H}{R}\right)t.$$

Upon integration twice and the insertion of the initial conditions, this yields

$$x = (v_H+R\omega)t - \frac{g_1}{6}\left(\omega+\frac{v_H}{R}\right)t^3.$$

When the projectile is at S (Fig. 1), near the end of its range, we have $t = T = 2v_V/g_1$ and

$$x = \frac{2(v_H+R\omega)v_V}{g_1} - \frac{4}{3}\frac{(\omega+v_H/R)v_V^3}{g_1^2}.$$

Comparison with Eq. (3) shows that in Fig. 1,

$$SE = \frac{4}{3}\frac{(\omega+v_H/R)v_V^3}{g_1^2}. \quad (5)$$

It would be easy to evaluate RG by employing the *total* time of flight in the foregoing substitution, but the difference between SE and RG is much too slight to justify the more cumbrous procedure.

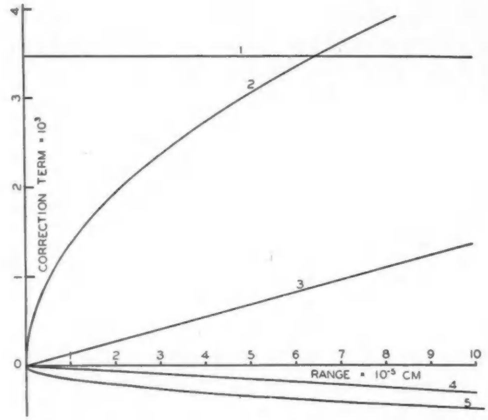

FIG. 2. Correction terms of Eq. (6) *versus* range of a projectile dispatched in the plane of the equator with positive (eastward) horizontal velocity and an angle of elevation of 30°. Numbers from *1* to *5* beside the curves are serial numbers of the correction terms following unity in the bracket of Eq. (6).

CORRECTED RANGE FORMULA

The observed range is the range of Eq. (4) diminished by the increment of Eq. (5). Thus:

Locally observed range =

$$\frac{2v_V v_H}{g_1}\left[1+\frac{R\omega^2}{g_1}+\frac{2\omega v_H}{g_1}+\frac{v_H^2}{Rg_1}-\frac{2v_V^2}{3Rg_1}-\frac{2v_V^2\omega}{3v_H g_1}\right]. \qquad (6)$$

The bracketed factor in this equation is of course always close to unity, and if the five small additive correction terms be neglected there remains only the range of Eq. (2). As to the properties of these terms, the first, $R\omega^2/g_1$, is a constant with no dependence upon projectile speed or initial direction. As such it may well remain attached to the elementary range formula even when the other correction terms are dropped. If it is to be retained it will be economical to rearrange as follows:

$$\frac{2v_V v_H}{g_1}\left[1+\frac{R\omega^2}{g_1}\right]\cong\frac{2v_V v_H}{g_1-R\omega^2}\cong\frac{2v_V v_H}{g},$$

a familiar result which was introduced in Eq. (1). This term $R\omega^2/g_1$ results from the downward (negative y) displacement of the landing area during the flight of the projectile.

The second term after unity in the brackets is caused by the tilting of the landing area during the projectile's flight. The landing area tilts downward on the east side, so an eastbound projectile will gain extra range through this term. Conversely, a westbound projectile (as locally regarded) will lose range as the negative value of v_H in the term would show. To isolate these gains and losses for observation, the area of experiment should be a plane surface normal to that radius of the earth which contains the point where the projectile is to be launched.

If the area of experiment conforms to the spherical surface of the earth rather than to a tangent plane, all projectiles in whatever directions bound will have additional time to fall and therefore additional time in which to acquire range. The third correction term in the bracket of Eq. (6) expresses the extra range so gained. Evidently the term is positive, whatever the direction of projection. The spherical surface separates itself from the plane at an increasing rate as one progresses away from their common point, so this source of extra range becomes increasingly effective as range increases; and the term varies with the square of the horizontal velocity. This term does not contain ω; it would be effective on a stationary earth.

The fourth correction term in the bracket is associated with the backward component of gravitational force which results from the displacement of the projectile from its point of projection in space by virtue of its locally imparted velocity. This term has nothing to do with the rotation of the earth but much to do with the duration of the flight, as the v_V^2 indicates. It is also affected by the size of the earth in that the term would vanish on an earth of infinite radius and therefore of uniform gravitational field. On the real earth the term is always negative, reducing the range.

The final term in Eq. (6) results from the backward component of gravitational force associated with the departure of the projectile from its space origin by reason of terrestrial rotation. This component is always westward, reducing eastward ranges as the negative sign preceding the term shows, and similarly extending westward ranges as v_H in the denominator is then negative and changes the sign of the term.

An idea of the magnitude of these terms will be conveyed by Fig. 2, where all are plotted as functions of range for the special case of projection at an angle of elevation of 30°.

ANY LATITUDE; ANY DIRECTION OF PROJECTION

The foregoing methods may now be somewhat generalized and extended to cover the case of a projectile dispatched from a point on the globe at any latitude λ and in a trajectory whose plane is directed in any desired azimuth. The angle μ will be taken as increasing positively from the north point clockwise through east as shown in Fig. 3.

The general projectile range achieved and measurable upon a rotating earth differs from the range that would be observed upon the same earth at rest for two reasons. First, the earth's rotation changes the general direction of the gravitational force upon the projectile in such a sense that the projectile is drawn somewhat westward. Second, rotation during the flight displaces the surface upon which the projectile must alight.

The first effect may be treated as in the equatorial case except that in the present general case the horizontal component of the gravitational force acting upon the projectile along the direction of the range is

$$-mg_1 \sin\left(\frac{v}{R}+\omega \cos \lambda \sin \mu\right)t.$$

Upon integration this gives a range increment,

$$-\frac{4v_V{}^3 v_H}{3g_1{}^2 R}-\frac{4v_V{}^3\omega \cos \lambda \sin \mu}{3g_1{}^2}.$$

The first term here does not contain ω; it appeared as the fourth correction term of Eq. (6), and its import was discussed in that connection. The second term in the expression corresponds to the final correction term of Eq. (6) and reduces thereto in the equatorial case.

The second way in which terrestrial rotation affects projectile range requires more discussion. Imagine a catapult mounted upon the flat deck of an airplane carrier for the conduct of experiments with projectiles whose ranges are insufficient to carry them beyond the plane surface of the deck. Neglecting for the moment any motions of the earth, one concedes at once that the ranges measured along the deck of the ship will be independent of any *uniform* motion the ship may possess along its course. As to nonuniform motions we need only inquire whether during the flight they have been such as to displace the landing spot from the position which it would have occupied at the concluding instant of the flight had the ship continued to move with the velocity it possessed when the flight began. If such a displacement has occurred the measured range will, in general, be affected.

It is convenient and almost necessary to consider separately the components of such a displacement, taken horizontally and vertically in the plane of the trajectory. (The horizontal component at right angles to the trajectory will not affect the range.) If D_H represents the horizontal component of the displacement, taken as positive in the sense of the horizontal motion of the projectile, the positive range increment is obviously $\Delta R_H = -D_H$. If D_V represents the vertical component (positive downward), it is evident from Fig. 4 that it will produce a positive range increment, $\Delta R_V = D_V \cot \alpha = D_V v_H / v_V$, where v_H and v_V are the component velocities imparted by the catapult. The notation of this

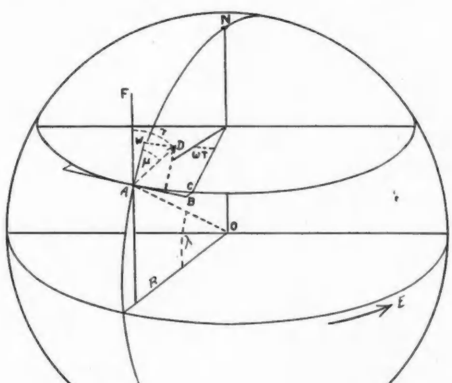

FIG. 3. Geometry of projectile motion on a rotating earth. Starting at A in latitude λ, the projectile is impelled toward D in a direction defined by the azimuth angle μ. During flight the earth rotates through the angle ωT.

illustration will be adopted in discussing projectiles for which the moving surface of the earth takes the place of the ship.

In this case then,

$$\text{Observed range} = \text{static-earth range} - \frac{4v_V^3\omega\cos\lambda\sin\mu}{3g_1^3} + \Delta R_H + \Delta R_V. \quad (7)$$

In Fig. 3 the point A represents the starting point of the trajectory and D its approximate terminus. The field of operations is shown as a rectangular area tangent to the sphere of the earth at A. During the flight of the projectile this area will have rotated about the earth's axis through an angle ωT and the point A will have advanced to the position indicated in Fig. 3 by C. It will be useful to decompose the motion of the area into a translation at the rate $R\omega$ along the tangent AB followed by a translation perpendicular to AB along BC, followed by a rotation through the angle ωT about an axis parallel to that of the earth and passing through the starting point of the trajectory, then at C. The first of these component motions is without effect upon the measured range since it is merely a continuation of the linear motion possessed by the point of departure at the instant when the projectile was released. The translation in the direction BC will in general have components along the direction of the range and along the vertical (parallel to AO). It will therefore contribute to both ΔR_H and ΔR_V. The postulated rotation about the starting point of the flight cannot give the end point any component of displacement along the range, but it will in general have a vertical component and will therefore contribute to ΔR_V though not to ΔR_H.

CALCULATION OF ΔR_H

Applying the sagitta relation again one readily finds that $BC = \frac{1}{2}R\omega^2T^2\cos\lambda$ to the required precision. It is now necessary to evaluate the component of BC lying in the direction AD of the trajectory, an operation that may be performed in two steps. The projection of BC upon an extension of the plane AWD will clearly lie in the direction AW and possess the magnitude $BC\sin\lambda$. The component of this projection lying in the direction AD is then

$$\Delta R_H = -BC\sin\lambda\cos\mu = -\tfrac{1}{4}R\omega^2T^2\sin 2\lambda\cos\mu$$
$$= -R\omega^2v_V^2\sin 2\lambda\cos\mu/g_1^2.$$

CALCULATION OF ΔR_V

The point where the projectile is destined to land undergoes a vertical displacement during the flight. This statement does not mean that the point in question changes its distance from the center of the earth, but that it changes its distance from that plane in fixed space which was also the horizontal plane through the starting point of the trajectory at the instant when the flight of the projectile began.

The contribution to D_V of the translation in the BC direction is the component of BC in the radial (locally vertical) direction AO; thus it is $2R\omega^2v_V^2\cos^2\lambda/g_1^2$. The contribution to D_V of the rotation requires resolution in the same vertical direction of the circumferential displacement of the end point of the trajectory in its postulated rotation about an axis through the starting point.

With further reference to Fig. 3, this rotation may be thought of as taking place about an axis such as AF, and for the present argument it is not essential to imagine the whole scene of operations moved over to C. Imagine the rotation about AF decomposed into two component rotations about axes through A. Let one axis, OA, be vertical and the other, AW, be horizontal and in the meridian plane. The rotation of AD about the vertical axis cannot alter the vertical position of D, so this component may be ignored. The rotation about AW proceeds at the rate $\omega\cos\lambda$, since ω is the angular velocity about AF and λ is the angle WAF. In time T the angle thus generated is $\omega T\cos\lambda$, and the consequent ver-

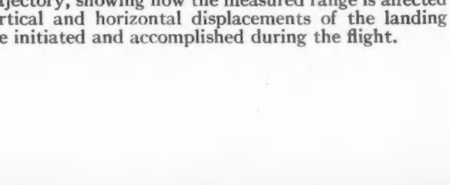

FIG. 4. Representation of conditions at the terminus of the trajectory, showing how the measured range is affected by vertical and horizontal displacements of the landing surface initiated and accomplished during the flight.

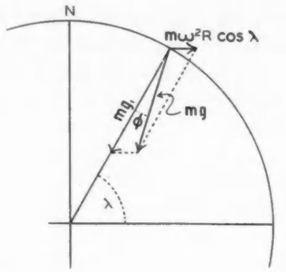

FIG. 5. Observed weight mg shown as the vector sum of Newtonian attraction and centrifugal force.

tical displacement of D is $WD\omega T \cos\lambda$, or $AD\omega T \sin\mu \cos\lambda$. Upon replacing AD and T by their now familiar equivalents, one obtains for the *total* normal displacement of the landing surface,

$$D_V = \frac{1}{{g_1}^2}(2R\omega^2 v_V{}^2 \cos^2\lambda + 4v_V{}^2 v_H \omega \cos\lambda \sin\mu).$$

The range increment resulting from this displacement is

$$\Delta R_V = D_V \frac{v_H}{v_V} = \frac{1}{{g_1}^2}(2R\omega^2 v_V v_H \cos^2\lambda + 4v_V v_H{}^2 \omega \cos\lambda \sin\mu).$$

Inserting in Eq. (7) the values of ΔR_H and ΔR_V, one obtains

$$\text{Observed range} = \frac{2v_V v_H(1+\epsilon)}{g_1} + \frac{2R\omega^2 v_V v_H \cos^2\lambda}{{g_1}^2} + \frac{4v_V\omega\cos\lambda\sin\mu}{3{g_1}^2}(3v_H{}^2 - v_V{}^2) - \frac{R\omega^2 v_V{}^2 \sin 2\lambda \cos\mu}{{g_1}^2}. \quad (8)$$

The small quantity ϵ, introduced to take care of surface curvature and plumb line convergence, consists of appropriate terms from Eq. (6); that is,

$$\epsilon = \frac{v_H{}^2}{Rg_1} - \frac{2v_V{}^2}{3Rg_1}.$$

The approximations adopted in deducing these results find justification in the smallness of ω and of ωT with respect to unity. They are excellent approximations for projectiles dispatched from this planet. The procedure with respect to these approximations has been equivalent to regarding all terms in ω^2 (except in the product $\omega^2 R$) as negligible in comparison with terms in ω.

REAL RANGE IN RELATION TO OBSERVED g

It may seem inappropriate to relate the real range, as we have here, to an approximate range formula containing two constants based upon unrealizable conditions. The gravitational acceleration g_1 is the acceleration that *would be observed* if the earth ceased rotating. The angle of elevation α is the complement of the angle between the direction of the initial velocity and the direction in which plumb lines *would* hang if the earth ceased rotating; this is *not* the angle of elevation observed on the real rotating earth because real plumb lines are alined not merely by Newtonian gravitation but in part also by centrifugal force.

It is of interest then to display the real range of Eq. (8) in a form having as its first approximation the quantity $V_0{}^2 \sin 2\beta/g$, where g is the ordinary and directly observable acceleration, and β is the complement of the angle between the initial direction of the projectile motion and the real plumb line. In the approximate range expression, $V_0{}^2 \sin 2\alpha/g_1$, the denominator g_1 may be replaced by $g + R\omega^2\cos^2\lambda$. Hence,

$$\frac{V_0{}^2\sin 2\alpha}{g_1} \cong \frac{V_0{}^2\sin 2\alpha}{g+R\omega^2\cos^2\lambda} \cong \frac{V_0{}^2\sin 2\alpha}{g}\left(1 - \frac{R\omega^2\cos^2\lambda}{g}\right) = \frac{V_0{}^2\sin 2\alpha}{g} - \frac{R\omega^2 V_0{}^2 \sin 2\alpha \cos^2\lambda}{g^2}. \quad (9)$$

Figure 5 portrays the real force of gravity on mass m as the vector sum of the Newtonian attracting force and the centrifugal force $mR\omega^2\cos\lambda$. Relations in the vector triangle show that $\sin\phi = R\omega^2 \sin\lambda\cos\lambda/g$, the angle ϕ lying in the plane of the meridian. Moreover, since ϕ is always small, Fig. 5 shows that $g_1 = g + R\omega^2\cos^2\lambda$

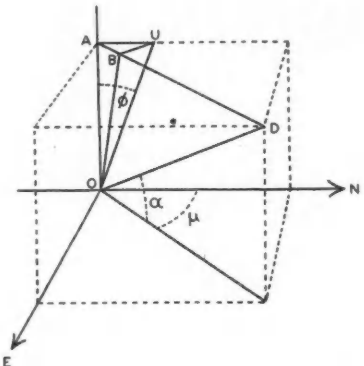

FIG. 6. Geometric relations for a projectile launched in the direction OD. The Newtonian acceleration is directed along AO, collinear with the earth's radius and normal to the horizontal plane of operations. The observed acceleration along UO follows the plumb-line direction and diverges from AO by an angle ϕ lying in the meridional plane.

as stated without demonstration on an earlier page.

Evidently the angle ϕ is zero at the poles and equator, and centrifugal force is without effect upon the *direction* of gravity at those places. In intermediate latitudes, ϕ is responsible for an extension or curtailment of the projectile range which must be applied in addition to all the other effects of rotation just discussed if and only if we would express the angle of elevation with respect to the observed plumb line rather than to the radius of the globe.

In Fig. 6, O is a point on the earth's surface from which a projectile is launched in the direction OD. The axis OA is an extension of the earth's radius, while UO lying in the same meridional plane is the acceleration vector g. This acceleration has a horizontal component which will necessarily extend or reduce the range. The total acceleration $UO[=g]$ is resolvable into the orthogonal components indicated by the vector equation $\mathbf{UO}=\mathbf{UB}+\mathbf{BA}+\mathbf{AO}$. In Fig. 6, $BA=AU\cos\mu$, and $AU=UO\sin\phi$, hence $BA=UO\sin\phi\cos\mu=g\sin\phi\cos\mu$.

The acceleration **BA** detains the projectile, reducing the range in the amount $\frac{1}{2}(BA)T^2$, where, as before, T is the time of flight. The decrement so incurred is $(2v_V{}^2R\omega^2/g^2)\sin\lambda\cos\lambda\cos\mu$, a quantity that changes sign when the direction of projection crosses the parallel of latitude through the point of origin. This lack of symmetry follows from the fact that the gravitational force on a rotating earth is not normal to the plane of operations, which is still a plane tangent to the spherical earth at O. In consequence of the angle ϕ we have then,

$$\frac{V_0{}^2\sin 2\beta}{g}=\frac{V_0{}^2\sin 2\alpha}{g}-\frac{2v_V{}^2R\omega^2}{g^2}\sin\lambda\cos\lambda\cos\mu.$$

Combining this result with Eq. (9), we have

$$\frac{V_0{}^2\sin 2\alpha}{g_1}=\frac{V_0{}^2\sin 2\beta}{g}+\frac{2v_V{}^2R\omega^2\sin\lambda\cos\lambda\cos\mu}{g^2}-\frac{R\omega^2V_0{}^2\sin 2\alpha\cos^2\lambda}{g^2}. \quad (10)$$

Returning to Eq. (8) and replacing the first term of the right-hand member by the right-hand member of Eq. (10), one obtains[1] after rearrangement and further rejection of small quantities,

$$\text{Range}=\frac{V_0{}^2\sin 2\alpha(1+\epsilon)}{g}+\frac{4\omega V_0{}^3}{3g^2}\sin\alpha[4\cos^2\alpha-1]\cos\lambda\sin\mu, \quad (11)$$

where g is the ordinary acceleration due to weight, V_0 is the initial velocity of the projectile, $\alpha[=\tan^{-1}v_V/v_H]$ is the initial angle of elevation (measured upward from the horizontal in the direction of projection), ω is the angular speed of rotation of the earth (rad/sec), λ is the geographic latitude of the point of departure of the projectile and μ is the azimuth of the plane of the trajectory, measured clockwise from the north point.

The quantity ϵ was defined in terms of g_1, but it is practically immaterial whether g_1 or g be used in calculating ϵ for use in Eq. (11). Indeed, the compact form of Eq. (11) was obtained by recognizing the distinction between g and g_1 only

[1] Derivations of this formula with ϵ neglected have been given by O. v. Eberhard, Zeits. f. Physik **60**, 528 (1930); and by W. H. Roever, *The weight field of force of the earth* (Washington University Studies, New Series, Science and Technology, No. 11, 1940) and Science **97**, 115 (1943). See also E. J. Routh, *Advanced rigid dynamics*, p. 29.

in those terms that express first approximations to the whole range, and ignoring the difference in relatively small correction terms.

The two terms within the brackets of Eq. (11) indicate that the whole correction is resolvable into two parts of opposite sign. The negative part tends to bring the projectile to earth at a point westward from where it would have landed in the absence of rotation, this statement holding true whether the local direction of propulsion was eastward or westward. Correspondingly, the positive term in the correction tends to shift the landing point eastward, regardless of the azimuth of the flight. The two opposed parts are equal in absolute value when $\alpha=60°$; for this angle of elevation the elementary range expression $V_0^2 \sin 2\alpha/g$ is correct and the terrestrial rotation is without net effect. The elementary expression is at its worst—that is, the absolute value of the required correction is maximum—for $\alpha=30°$.

The correction makes its largest showing, everything considered, in the case of eastward or westward projection at the equator when $\alpha=30°$. For this case the ratio of the whole correction to the range is $8\omega V_0/3^{\frac{3}{2}}g$, or approximately $1.15\times10^{-7}V_0$, the speed being in centimeters per second.

The approximations involved in the foregoing derivations are least objectionable for projectiles of low initial velocity. Slow projectiles also experience the minimum air resistance and are least affected by variations of g_1; so, for such cases Eqs. (8) and (11) are dependable without further adjustment except that one may put $\epsilon=0$.

A Laboratory Experiment on the Band Spectrum of Fluorine

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DETERMINATION of the angular momentum of the fluorine nucleus by measuring a molecular band spectrum plate of fluorine has proved to be a popular and instructive experiment in a course in experimental nuclear physics given at the Massachusetts Institute of Technology. The present paper describes the way in which the experiment is set up and how it is carried out by the students. This is preceded by a brief review of the theory of the alternating intensities of band spectra.

THEORY

Let us consider a molecule consisting of two identical nuclei, that is, a homonuclear diatomic molecule. The wave function containing the coordinates of all the particles of the molecule is

$$\psi=\psi(x_1, y_1, z_1, s_1, \cdots, x_i, y_i, z_i, s_i),$$

where the x's, y's and z's refer to the three spatial coordinates, and the s's refer to the spin coordinates of the particles. Numbered among the particles are all the electrons and both nuclei.

The probability of finding the system in a state with the coordinates of the individual particles between limits x_1 and $x_1+dx_1\cdots s_i$ and s_i+ds_i is

$$\psi\psi^* dx_1\cdots ds_i.$$

Since there is no way of distinguishing between two identical particles, $\psi\psi^*$ must remain unaltered if the coordinates of the two nuclei are interchanged; ψ itself must remain unaltered in magnitude, but it can change sign. When the coordinates of the nuclei are interchanged and ψ remains unaltered in sign, the state is said to be *symmetric* in the nuclei. When the coordinates of the nuclei are interchanged and ψ changes sign, the state is said to be *antisymmetric* in the nuclei. One can show that it is impossible to have any transitions between two molecular states having ψ's of opposite symmetry in the nuclei.[1] If we consider homonuclear molecules of a given isotope, the molecular states which occur in nature have ψ's of only one symmetry. Experiments indicate that nuclei made up of odd numbers of fundamental particles have odd half-integral spins and may occur only in states of antisymmetric ψ, whereas nuclei made up of even num-

[1] G. Herzberg, *Molecular spectra and molecular structure* (Prentice-Hall, 1939), vol. I, pp. 138–139.

bers of fundamental particles have integral spins and may form only in states of symmetric ψ. Nuclei that may form only molecular states of antisymmetric ψ are found to obey the distribution called Fermi-Dirac statistics. Nuclei that may form only molecular states of symmetric ψ are found to obey Einstein-Bose statistics.

If the nuclei have a spin different from zero, the total wave function may be written,

$$\psi = u\cdot\beta + \delta,$$

where u is the wave function containing all the coordinates of the electrons but only the space coordinates of the nuclei, β is the nuclear spin function which depends on the orientation of the nuclear spins, and δ is a small perturbation term due to the magnetic interaction of the nuclear spin with the electronic angular momentum of the molecule.

It can be shown[2] that β is symmetric in the nuclei if **T**, the vector sum of the angular momentums of the two nuclei in the molecule, is given by $2I$, $2I-2$, $\cdots$ and that β is antisymmetric if **T** is given by $2I-1$, $2I-3$, $\cdots$, where I is the intrinsic angular momentum ("spin") of each nucleus. Since, to a good approximation, $\psi = u\cdot\beta$, we have for the case of ψ antisymmetric, that the symmetry of u and β must be opposite. This means that if β is antisymmetric, u must be symmetric; and if β is symmetric, u must be antisymmetric. In a similar manner, if ψ is symmetric, then β and u must have the same symmetry. These results are summarized in Table I.

Table I shows that there are two types of wave function u—antisymmetric and symmetric in the space coordinates—for both the case when ψ is antisymmetric and when ψ is symmetric. It will now be shown that the transition probability between u's of different symmetries is very small but is finite and that molecular states having u's of either symmetry should occur in nature with abundances in a fixed ratio.

TABLE I.

Statistics	ψ	β		u
Fermi-Dirac	antisym.	antisym. sym.	$2I-1, 2I-3, \cdots$ $2I, 2I-2, \cdots$	sym. antisym.
Einstein-Bose	sym.	antisym. sym.	$2I-1, 2I-3, \cdots$ $2I, 2I-2, \cdots$	antisym. sym.

[2] For example, follow arguments similar to those presented for the two electrons in He; see Herzberg, *Atomic spectra and atomic structure* (Prentice-Hall, 1937), p. 124.

Although we have no transitions between states whose total wave functions ψ have different symmetries, let us consider the transition probability between states where both ψ_n and ψ_m have the same symmetries, but where u_n and u_m have different symmetries. (β_n and β_m will have different symmetries, so ψ_n and ψ_m have the same symmetry.)

The transition probability is proportional to the square of the matrix element. The matrix element for a transition between two states m and n whose total ψ's have the same symmetry is given by the expression,

$$\int \psi_n \psi_m^* q d\tau = \int (u_n\cdot\beta_n + \delta_n)(u_m^*\cdot\beta_m^* + \delta_m^*) q d\tau,$$

where q is the interaction term—for example, dipole moment for dipole transitions—$d\tau$ is the volume element $dx_1\cdots ds_i$, and the integration is carried out over the entire coordinate space. If we carry out the multiplication indicated in the right-hand member of this equation, the term not involving δ will be ruled out since u_n and u_m, likewise β_n and β_m, have different symmetries. Terms are still left that involve δ, and therefore the matrix element may be different from zero. However, owing to the small magnetic moment associated with the nuclear spin, the interaction term δ is very small, and hence the transition probability between states whose u's are of different symmetries is very small and the mean life for such a transition is of the order of months or years. Since transitions can take place, both states do occur in nature and occur with an abundance in the ratio of the statistical weights of the states. The statistical weight of a state is obtained as follows. Let us consider a quantity **J** which is the vector sum of the electronic angular momentum (the component along the axis of symmetry of the molecule) and the angular momentum due to the rotation of the nuclei about an axis perpendicular to the line joining their centers. In a magnetic field, **J** can take only the directions for which the component in the field direction is M_J, where

$$M_J = J,\ J-1,\ J-2,\ \cdots,\ -J.$$

That is, there are $2J+1$ magnetic substates. As the magnetic field is reduced to zero, these $2J+1$

substates all tend to the same energy and hence are superposed. The resulting state is said to have a rotational statistical weight equal to the number of these superposed substates.

Let us consider states having a given value of **J**. Since the magnetic interaction of the nuclear moment is small, the energy of all such states is independent of **T**, the resultant of the intrinsic angular momentums of the nuclei.

We take the ratio of the sum of the statistical weights $2T+1$ for all the values of **T** in the case of symmetric β to the similar sum for all the values of **T** in the case of antisymmetric β. This ratio is found to be $(I+1)/I$.

Referring to Table I, we see that for the case of nuclei obeying Fermi-Dirac statistics, the antisymmetric β corresponds to a symmetric u, and symmetric β to an antisymmetric u. Hence the ratio of the statistical weights of states having antisymmetric u to that of states having symmetric u is $(I+1)/I$. In a similar way for the case of nuclei obeying Einstein-Bose statistics, the ratio of the statistical weights of states having antisymmetric u to that of states having symmetric u is $I/(I+1)$.

It will now be shown that the actual bands in a spectrum of a diatomic homonuclear molecule involve electronic transitions and have a fine structure. Furthermore, the fine structure components (lines) of each band alternate in intensity, and the ratio of intensities of the even- to odd-numbered lines is fixed.

Each line observed in a spectrum arises from a transition between two states. Transitions are governed by selection rules. It can be shown[3] that, for homonuclear molecules, transitions between rotation or vibration-rotation molecular states having the same electronic energy are impossible and hence that pure rotation or pure vibration-rotation spectra do not occur. However, transitions between rotational states having u's of the same symmetry may take place if the transition involves a change in the electronic energy. These transitions give rise to the fine structure of the bands in electronic spectra.

The intensity of a line is proportional to the statistical weights of the states involved in the transition. As previously shown, lines arise only from transitions between states having u's of the same symmetry. Hence the intensity of the lines arising from transitions between states having symmetric u will be proportional to the statistical weights of states having symmetric u. Similarly, lines arising from transitions between states having antisymmetric u will be proportional to the

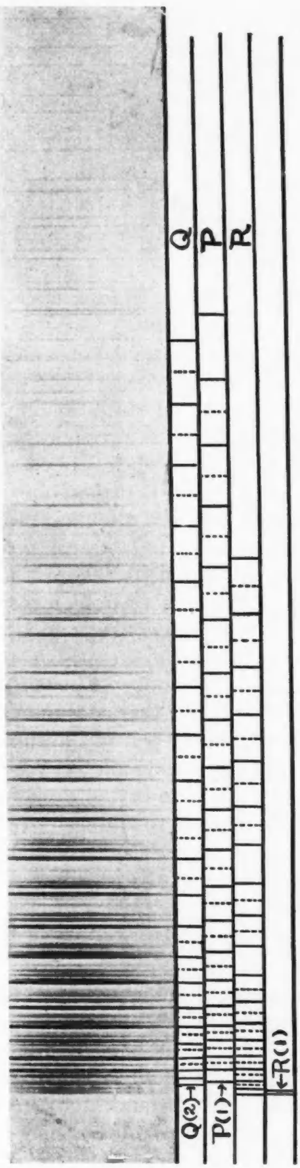

FIG. 1. Band spectrum of fluorine taken by Gale and Monk (see reference 1).

[3] G. Herzberg, reference 1, p. 138.

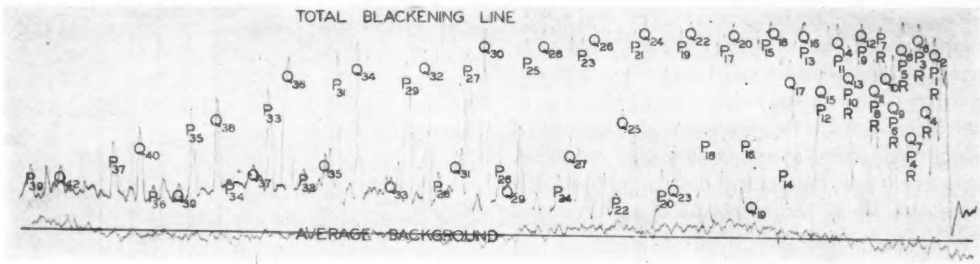

FIG. 2. Microphotometer trace of the band spectrum plate shown in Fig. 1.

statistical weights of states having antisymmetric u. We have shown previously that the ratio of the statistical weight of states having antisymmetric u to that of states having symmetric u is given by $(I+1)/I$ if the nuclei obey Fermi-Dirac statistics and by $I/(I+1)$ if they obey Einstein-Bose statistics. Therefore, the ratio of the intensity of the lines arising from transitions between states having antisymmetric u to that of the corresponding lines arising from transitions between states having symmetric u is given by $(I+1)/I$ if the nuclei obey Fermi-Dirac statistics and by $I/(I+1)$ if they obey Einstein-Bose statistics. It can be shown[4] that odd-numbered lines have u's of one symmetry and even-numbered lines, u's of the opposite symmetry. We conclude, therefore, that the ratio of the intensity of the even-numbered lines to the intensity of the odd-numbered lines is $(I+1)/I$ or $I/(I+1)$, depending on the particular statistics which the nuclei obey.

PREPARATION OF THE EXPERIMENT

For the purposes of this experiment, it is essential to obtain a reproduction of a band spectrum plate. In this laboratory, good results were obtained by photographing an excellent band spectrum plate taken by Gale and Monk.[5] A positive transparency made by photographing Plate II of Gale and Monk's paper has been found entirely satisfactory. Although this copy is certainly subject to errors in reproduction, the spin of the fluorine nucleus can be determined without ambiguity. Through the kindness of Professor Monk in lending us the original plate, this band spectrum is again reproduced in Fig. 1 of this paper. The preparation of the experiment involves photographing this reproduction of the band spectrum of fluorine, and obtaining a positive transparency of such a size that the entire number of identified lines can be measured in a single run of the particular microphotometer to be used.

The following copying procedure can be recommended. Obtain a soft full-scale negative by using Eastman "Commercial" film in photographing the halftone reproduction of the band spectrum in Fig. 1. In making a positive from this negative, Eastman "Process" film should be used to clear the whites and to permit development to a contrast that provides a faithful reproduction of the relative line intensities on the original halftone.

EXPERIMENTAL PROCEDURE

The positive reproduction of the band spectrum plate is set up in a microphotometer, and a trace of the lines is obtained. On the same record, the microphotometer should be re-run over a part of the film that was not exposed to the lines to get a background of the film itself. A second re-run should be made with the light beam of the microphotometer interrupted before it strikes the film, to obtain a "total blackening" line. Figure 2 is typical record from the microphotometer.

The student must now take this microphotometer trace and identify the peaks with the help of the analysis of the lines in Fig. 1. If the wave numbers of the lines shown in this band spectrum are plotted as a function of the numbers of the lines, curves similar to those shown in Fig. 3 are obtained. The data fall into three distinct groups forming what are known as the P-, Q- and R-branches of the spectrum. The notation P, Q and

[4] G. Herzberg, reference 1, pp. 260–263.
[5] H. G. Gale and G. S. Monk, Astrophys. J. **64**, 77 (1929).

R on the lines shows to which branch they belong, and the subscripts are the numbers of the lines.

The student should construct a table for the P- and Q-branches and only those lines of the R-branch that overlap either the P- or Q-branch or both. The lines in the R-branch are less intense than those in the P- and Q-branches, and it is not worth while to include the R-branch in the experiment.

The distance on the record from the peak of each line to the "total blackening" line should be recorded opposite the number of each line. This value is called E_m. An average line should be drawn through the background. At each peak, the distance should be measured between this background line and the "total blackening" line. This value is called E_0.

The relative intensity of the original light emitted will be[6]

$$E=(E_0/E_m)^{1/\gamma},$$

where γ is the resultant slope of the characteristic, or "H and D," curves of the photographic emulsions used. Fortunately for this experiment, the value of γ is not critical. An unambiguous answer to the spin of the fluorine nucleus may be obtained even if the value of γ is not known accurately. For the film reproduced in this laboratory and used in this experiment, $\gamma=2$.

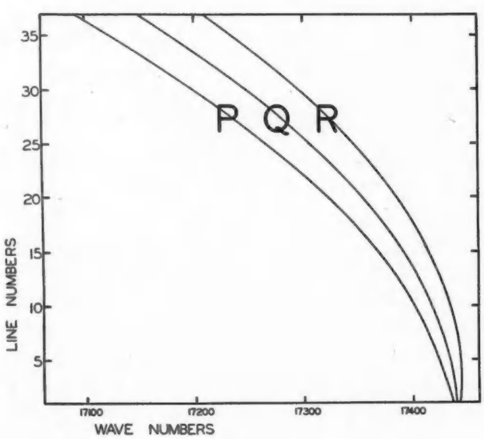

FIG. 3. Plate showing the "branches" in the band spectrum of fluorine.

[6] A. C. Hardy and F. H. Perrin, *The principles of optics* (McGraw-Hill, 1932), pp. 219–223.

Near the head of the bands, many of the lines are superposed. For the purpose of this experiment, it is sufficiently accurate to divide the total

TABLE II. Typical data from Fig. 2.

Line		E_m	E_0	E_0/E_m	E	$E/Line$
Q_2, P_1, R	Q_2, P_1	1.60	9.25	5.78	2.40	0.80
Q_4, R		1.70	9.25	5.44	2.33	1.16
Q_6, P_3, R	Q_6, P_3	0.40	9.24	23.1	4.81	1.27
Q_7, P_4, R	Q_7, P_4	2.00	9.23	4.61	2.15	0.72
Q_8, P_5, R	Q_8, P_5	0.39	9.23	23.7	4.87	1.62
Q_9, P_6, R	Q_9, P_6	2.22	9.22	4.15	2.04	0.68
Q_{10}		2.68	9.22	3.44	1.85	
P_7, R	P_7	0.61	9.22	15.1	3.89	1.94
P_8, Q_{11}, R		2.24	9.22	4.11	2.03	0.68
Q_{12}, P_9		0.40	9.21	23.0	4.81	2.40
P_{10}, Q_{13}, R_{18}	P_{10}, Q_{13}	1.77	9.21	5.20	2.28	0.76
P_{11}, Q_{14}		0.42	9.20	21.9	4.68	2.34
P_{12}, Q_{15}	P_{12}, Q_{15}	2.86	9.20	3.22	1.79	0.90
Q_{16}		0.33	9.20	27.85	5.28	
P_{13}		0.40	9.20	23.0	4.80	
Q_{17}		2.38	9.20	3.87	1.97	
P_{14}		6.41	9.20	1.43	1.20	
Q_{18}		0.35	9.20	26.27	5.13	
P_{15}		0.83	9.20	11.07	3.33	
Q_{19}		7.20	9.19	1.28	1.13	
P_{16}		5.36	9.19	1.71	1.31	
Q_{20}		0.42	9.19	21.9	4.68	
P_{17}		0.93	9.19	9.88	3.14	
P_{18}		5.79	9.19	1.59	1.26	
Q_{22}		0.40	9.18	22.1	4.70	
P_{19}		0.83	9.18	11.1	3.33	
Q_{23}		6.91	9.18	1.33	1.15	
P_{20}		6.83	9.17	1.34	1.16	
Q_{24}		0.44	9.17	20.85	4.57	
P_{21}		0.96	9.17	9.55	3.09	
Q_{25}		4.60	9.16	1.99	1.41	
P_{22}		7.77	9.16	1.18	1.09	
Q_{26}		0.64	9.16	14.3	3.78	
P_{23}		1.30	9.15	7.03	2.65	
Q_{27}		5.97	9.15	1.53	1.24	
P_{24}		6.00	9.15	1.525	1.23	
Q_{28}		0.81	9.15	11.3	3.36	
P_{25}		1.46	9.15	6.27	2.50	
Q_{29}		6.69	9.14	1.37	1.17	
P_{26}		6.47	9.14	1.41	1.19	
Q_{30}		0.74	9.13	12.35	3.51	
P_{27}		2.13	9.13	4.28	2.07	
Q_{31}		5.65	9.13	1.62	1.27	
P_{28}		6.06	9.12	1.50	1.22	
Q_{32}		1.76	9.12	5.18	2.28	
P_{29}		2.30	9.12	3.97	1.99	
Q_{33}		6.49	9.12	1.41	1.19	
Q_{34}		1.43	9.11	6.37	2.52	
P_{31}		2.04	9.11	4.47	2.11	
Q_{35}		5.98	9.11	1.52	1.23	
P_{32}		6.41	9.10	1.42	1.19	
Q_{36}		1.86	9.10	4.89	2.21	
P_{33}		3.91	9.10	2.32	1.52	
Q_{37}		6.16	9.10	1.48	1.22	
P_{34}		6.92	9.10	1.31	1.14	
Q_{38}		3.78	9.09	2.40	1.55	
P_{35}		4.22	9.09	2.16	1.47	
Q_{39}		6.29	9.08	1.44	1.20	
P_{36}		6.75	9.08	1.34	1.16	
Q_{40}		4.55	9.07	1.99	1.41	
P_{37}		5.45	9.07	1.66	1.29	
Q_{42}		6.07	9.07	1.49	1.22	
P_{39}		6.30	9.07	1.44	1.20.	

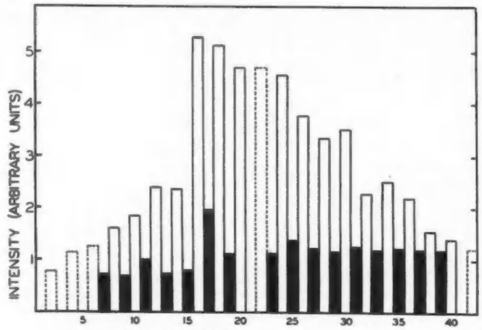

FIG. 4. Plot of the intensities in the Q-branch.

measured intensity by the number of superposed components to determine the intensity of a line in each particular branch. A typical set of data measured and calculated from the microphotometer trace of Fig. 2 is shown in Table II.

Experimental inaccuracies cause the individual ratios of the intensities of successive lines to fluctuate about the mean value. The method of averaging described below is satisfactory and involves less calculation than averaging the individual ratios. After all the intensities have been computed for the P- and Q-branches, a plot of the intensities should be made. Histograms of the odd-numbered lines and of the even-numbered lines are constructed by plotting the intensities as a function of the line numbers. There are some lines missing because of special selection rules. When this is the case the corresponding line of the opposite symmetry should be left out of the calculation. Figure 4 is a plot of the intensities of lines in the Q-branch from the data of Table II. It will be noticed here that lines 1, 3, 5, 21 and 41 are missing; hence lines 2, 4, 6, 22 and 42 should be omitted from the calculations. The ratio of the area of the histogram of the odd lines to the area of the histogram of the even lines is the average of the ratios of the relative intensity of successive lines. The spin I of the nucleus can then be calculated from the expression,

$$\text{ratio of areas} = (I+1)/I.$$

For the particular plot of Fig. 4, the ratio of the area of the plot for the even lines to the area of the plot for the odd lines comes out to be 2.8, which corresponds to a spin $I = \frac{1}{2}$. This is the accepted value for the spin of the fluorine nucleus.

Education of Physicists for Industry

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THE American Institute of Physics in a report issued in April, 1941 stated that there were in this country approximately 4500 physicists and that about half of that group were employed in industrial laboratories and half in the teaching profession. It is probable that the teaching profession is much nearer the saturation point than are the industrial laboratories and that this proportion will very materially change in the future. This condition very naturally brings up the question as to whether these two types of physicists should be given the same initial training. Inasmuch as the present type of training was developed primarily for the physicist who planned a teaching career, the problem arises as to how best to train a physicist for industry.

The best training course for a physicist naturally involves the training that will provide the best foundation for his job. Since there are wide varieties of industries and the physical problems involved are so numerous and varied, it would be futile to attempt to train any one physicist in all the different specialized fields encountered in industries. Consequently, he must be given the training requisite for coping in the future with problems that he has not previously encountered. That, in a broad sense, is what we mean by the research type of training. Accordingly, industrial requirements for a physicist must include graduate work.

Before discussing the training any further, we should consider the type of work that the man is to be associated with, what he is expected to do, the problems he faces. To illustrate my reasoning

I must confine my specific remarks to the glass manufacturing industry. Something similar in nature, though greatly different in detail, exists in all industries; however, in some the problems are more highly specialized than in the glass industry.

Glass manufacturing is probably more generally thought of as a chemical industry, yet the problems encountered in making and in using glass are largely physical. I will briefly outline the physical nature of the manufacture and use of glass so that the problems will be apparent and, hence, perhaps give a better idea as to the type of training needed.

In glass making, the raw materials—usually silica and the proper fluxing agents—are first mixed together, and then the mixture is put in a suitable container or pot where it is melted and worked. That is the operation in short. Now let us analyze the processes physically. How shall the glass be mixed? Melting is a solution of refractory particles in a flux having low melting point. The flux must be kept in contact with the refractory particles in order to be effective. How is this best accomplished? The grain size should be small for rapid solution, but then the dry mixture becomes almost thermally nonconducting. So at once we face a physical problem of the best combination or compromise among grain size, uniformity of mix and thermal conductivity—the problem of rapid solution and efficient heat transfer. If the glass is melted in a continuous tank, as most of it is, at what temperature shall the tank be fired? What are the relationships between the temperature of firing and the time of melting and planing the glass? Planing has to do with the rate of elimination of occluded gas bubbles from the melt. Can the phenomena be associated in a quantitative way with the viscosity of the glass? We are now confronted with the problem of measuring the melting time and the planing time of a glass as a function of temperature and composition, and of associating these phenomena with a physical property of the glass, namely, its viscosity. Thermal convection currents enter to affect the mixing of molten glass and to influence the erosion of the enclosing pot walls. This is a physical phenomenon controlled by the variation in the specific gravity of molten glass as a function of temperature. There are so many physical phenomena entering the process that I could write many pages on this phase alone. The application of the laws of heat transfer by thermal radiation is important; the radiation transmission properties of the glass, as affected by the temperature of the glass, the flames and the side walls all are tremendously effective. The surface tension of the glass affects the rate of wall solution. The rate of volatilization of the flux constituents of the glass as a function of temperature and of flame velocity have a close bearing on the final quality of the glass. These phenomena introduce the problem of measuring vapor pressures as functions of temperature and of composition.

The problem of working and shaping glass is strictly physical in character, since it is entirely a matter of viscous flow under applied forces. Surface tension is an important shaping agency. The rate of cooling during forming limits the shaping. Annealing is a problem of viscous flow, and its operation is merely one of proper temperature control. The laws of annealing may be worked out by mathematics, but the physical constants of the equation must be measured by experiment and the validity of the analysis checked.

The uses to which glass can be put are controlled and determined by its physical properties. Among the properties of interest are strength, coefficient of elasticity, thermal expansivity, thermal conductivity, specific heat, specific gravity, mechanical hardness, electric conductivity, dielectric strength, dielectric constant, power factor, together with the entire range of optical characteristics. Tests must be devised to subject articles made from glass to severe conditions simulating use. Is the article good, and, if so, how good?

There are in most cases no standardized tools for making measurements of the physical phenomena and properties in which we are interested. Since a quantitative determination is needed, a physicist must analyze the situation, select or design the instruments for measurement, make the measurements and then interpret the results.

Since it would appear that the general situation here is the same as that in any research problem, our educational problem is one of

determining how best to educate and train a research man. Should it be a sink-or-swim program or a planned course of instruction? Fortunately, almost anything works with certain men.

Let us consider the present type of advanced training course. It consists of lectures and a research course based entirely on one thesis problem. I will not criticize the lecture courses. They are in the main useful; if not, they will soon be forgotten. I believe the average doctoral thesis problem fails to provide adequate training for industrial research and the undergraduate courses fail to furnish the experimental background required.

I am of the opinion that the usual type of doctoral research—often using standardized equipment of a very special type—is too limited in its scope to develop either the latent research ability of a man or even to indicate whether it is present. It does not cover a broad enough field of experimental physics so that the student has an opportunity to learn the tools of other fields. The problems are often selected and directed by a department specializing in a certain field, and the results are planned to augment the previous work of this department. I believe anyone will agree that this is true. One physics department headed by a man who has specialized in series spectra will assign its students to spectral measurement problems; another department with high voltage equipment will encourage atom smashing. Understand, I do not find fault with these studies except as they may be the "major" part of a training course for an industrial physicist. Then what is better? I propose a new degree: call it Doctor of Applied Physics, or Doctor of Physical Engineering. I would substitute for the Ph.D. research thesis at least a three-year course of broadly planned experimental physics. Some or most of these experiments should be of such a nature that the experimenter constructs his own equipment just as he may now do in an original research. The experiments could duplicate some of the more important researches of recent times. While the purpose of this program is to develop a high degree of general experimental competence, there should be some specialization and the relative amounts of specialization in particular fields should be determined by a market analysis of the requirements for physicists in these fields. Such an analysis should reveal the average number of new men required per year by the various industrial and agency laboratories and in just what lines these men should have specialized.

Contrasted to this idea of a general experimental and research training course is that of having highly specialized courses designed to fit the needs of special industries. An analysis of the number of physicists needed by these industries would serve as a basis for determining the types of specialization required and the number of men per year who could be placed after graduation. Such specialized training probably could be efficiently carried out by assigning the training courses for certain industries to certain schools. A certain institution might go in for specialized optical training, another for general physics or properties of matter, another, high frequency studies and vacuum tube circuits, and so forth. They would be in the position of attempting to meet a definite, known demand. The question of demands would be relatively easily answered; in fact, the American Institute of Physics has some of this information available at the present time. In other words, the training should be controlled by a market estimate of the requirements. This plan of a division along institutional lines should lead to increased efficiency. Just as one factory can more efficiently make lawn mowers by making only lawn mowers and another manufacturer makes the cheapest and best washing machines by confining himself to washing machines, so should one institution be better able to train men along a specialized line by concentrating on that particular line. However, I am not sure that I am particularly enthusiastic about such a division. One danger is that it might tend to defeat the broader training in experimental physics that I believe to be highly desirable for each man.

There is now a tendency on the part of certain industries to set up their own special training centers. The Institute of Paper Chemistry at Appleton, Wisconsin is an example of this. Owing to the success of this venture other industries are thinking seriously of something similar. There is little doubt that the limited opportunities offered for securing men specially trained in the field of a certain industry are the

major inducement to a manufacturer to invest his money in the support of such a training laboratory. Certainly such work should be very carefully followed and watched. It may grow and may be partially adopted by other institutions and industries. *Such a movement is based on the failure of our present educational system to meet the demands of the industries.*

Support of industrial fellowships in our university laboratories is not the answer to this problem. I doubt whether these have been satisfactory either to the universities or to the sponsors, either as a method of getting an experimental problem solved or as a training course for a future industrial physicist.

Furthermore, in addition to the purely technical training in physics and the associated subjects such as mathematics, mechanics and chemistry, I would advise that additional requirements include courses in:

Business administration;
Patent law;
History of industrial growth (so far as I know no such history has as yet been written);
Management, labor laws and labor relationships;
Fundamental economics.

A certain portion of these secondary courses could, no doubt, be made a part of the undergraduate curriculum.

This is at best a sketchy synopsis drawn from practical experience. No attempt has been made to complete any details of these plans. The general experimental and research training course could be set into operation without any radical changes in our present laboratory personnel or equipment. The planning of the courses would, I believe, be easily carried out after the data on the needs for physicists by the different types of industries had been properly analyzed and appraised.

Newton's Third Law of Motion as Presented in Textbooks of Physics

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An examination of textbooks of physics on the particular subject of Newton's third law of motion has revealed, in many instances, an apparent lack of appreciation of the very simple significance of this law. In some cases the law is made to say something that it does not say at all, and that in general is not true, while in others the author makes the law appear much more complicated and difficult to understand than it really is. Simply to criticize an author's language in a text is not my purpose here. Most of the textbooks in question are well known, some widely used, and the authors have deservedly good reputations. It is, therefore, all the more unfortunate that on this particular point students should be given erroneous ideas, which they carry with them in later work. I realize that from time to time in the past others[1] have called attention to essentially the same points, but their criticisms seem to have been overlooked or ignored.

A common translation of Newton's statement of the law is as follows: "To every action there is always an equal and contrary reaction; or, the mutual actions of two bodies are always equal and oppositely directed."

Let me state at once the manner in which the law is misunderstood and misinterpreted. The difficulty is manifested in two respects. First, it is apparently not realized that the forces of action and reaction are on different bodies, and not on the same body. Second, the equality of action and reaction referred to is confused with the condition for equilibrium of a body. The second error is a natural consequence of the first, for if forces of equal magnitude and opposite direction act on a body in the same straight line, the body will be in equilibrium so far as those two forces are concerned.

Perhaps the reader doubts that such ideas are actually expressed in textbooks of college physics,

[1] See, for example, G. S. Fulcher, "Can a body exert a force on itself?" Science **44**, 747 (1916); E. Laird, "Inertia reaction," Science **46**, 341 (1917); V. F. Lenzen, "The third law of motion," Am. J. Phys. (Am. Phys. T.) **7**, 134 (1939).

but I submit as proof direct quotations from several different books, nearly all of which are for college students. In order not to give the appearance of trying to discredit any particular author, I shall distinguish the quotations only by number.[2] Both authors and publishers have very kindly permitted such use of the quotations.

Of course it may be said that the explanation and illustrations given by Newton himself are not as clear as they have since been made by others. Actually he gave sufficient explanation to show the ideas he had in mind; and, as in the case of many other pioneers, succeeding generations, with the foundations laid for them, and with better development of the subject available, have been able to improve on the clarity of the statements made by the discoverer or original author.

It should be said that many of the authors whose words are quoted make, elsewhere in their discussion of the same subject, statements that are entirely proper and excellent and hence by contrast seem strange in such close association with the remarks which are so open to criticism. It is indeed sometimes difficult to see how the same author could write both statements.

Let us now consider some specific examples of statements taken from current textbooks.

> (*1*) If a horse exerts a 300-lb pull or force F, upon the rope attached to a canal boat a moment after starting, then the backward pull that the canal boat exerts upon the other end of the rope cannot possibly be either more or less than 300 lb. Many people think that the forward pull of the horse must be at least slightly greater than the backward pull of the boat or the latter would not move. Many also think that the winning party in a tug-of-war contest must exert a greater pull on the rope than does the losing party, *which is certainly not the case.* Imagine the absurdity of trying to pull with a force of 10 lb east on one end of a string and 9 lb west on the other end!

Several things are wrong here. The phrase "a moment after starting" is entirely superfluous, for Newton's law applies just as well a moment before starting, or an hour after starting. Since Newton's law refers to the actions of *two* bodies, it is undesirable to bring in a third, namely, the rope. The author thus speaks of the pull of the horse on the rope and of the pull of the boat on the rope. This is not an illustration of Newton's third law. As a matter of fact, if the rope has a mass m and the system has an acceleration a, then the pull forward on the rope by the horse *is* greater than the pull backward on the rope by the boat, by the amount ma. If the objection be raised that the horse, because of his limitations, cannot pull directly on the boat, then let the pull of the horse on the rope be the action and the pull of the rope on the horse be the reaction, or the pull of the rope on the boat be the action and the pull of the boat on the rope be the reaction. As to the absurdity of trying to pull with a force of 10 lb eastward on one end of a string and with 9 lb westward on the other end—so far from being absurd, it is very commonplace. If the rope had a mass of 1 lb—and this surely would not be excessive for a rope used in towing a boat or for a tug-of-war—then pulls of 10 lb eastward and 9 lb westward would give the rope an acceleration of 32 ft/sec^2 eastward (which is only the magnitude of the acceleration of a falling body).

(*2*) Since this sample is especially concerned with figures in the text, direct quotations will not be made. Newton's third law is illustrated by a tug-of-war with three men on each side. Inserted in the rope between them is a spring balance, and along the rope are two arrows, one pointing to the left, and the other, of equal length, to the right. The second picture shows that one man has slipped down, and in it the arrows are of different lengths; while in the third picture, when one side has been quite overcome, the arrow representing the pull of the vanquished side has diminished to about one half its first value, while the victorious side pulls with a greater force than at the beginning. As remarked under (*1*), if the rope and balance are being accelerated, then the winning side *will* be pulling more than the losing side by an amount equal to the total mass of the rope and spring balance multiplied by the acceleration, but this is a relatively small force and was evidently not in the author's mind. The figures and discussion give the impression that when two bodies are pulling (or pushing) each other, one of them may diminish its pull indefinitely while the other continues to pull as much as before—a flagrant violation of Newton's third law.

(*3*) Under a topic labeled "Third Law . . . Interaction," we find the following:

> In a tug-of-war one team is trying to pull the rope one way, and the other team is trying to pull the rope

[2] Except in one case, where the publisher has insisted that acknowledgment be made.

> the other way. When both teams exert the same force, the rope remains stationary but in a state of tension. . . . The train pulls back on the locomotive just as hard as the locomotive pulls forward on the train (when moving at a uniform speed).

There are three ideas conveyed here that are fundamentally wrong. (*i*) The two pulls of the two teams on a third body (the rope) are not action and reaction. (*ii*) It does not at all follow that the rope is stationary when both teams exert the same force. (*iii*) The pulls of the locomotive and of the train are equal in magnitude whether the train is moving at a constant speed or not.

(*4*) First there is a perfectly good statement: "The forward pull of the engine on a train is exactly equal to the backward drag of the train on the engine. . . ." This proper statement is then somewhat spoiled by the following:

> On first reading these statements one is likely to inquire concerning the causes of the motion if these statements are true. The train moves forward with increasing velocity because the pull of the engine on the train exceeds the drag of friction and air resistance on the train. The forces between the bodies of the system are balanced; but, acting on the system as a whole, there is an unbalanced force represented by the difference between the pull of the engine and the friction and air resistance.

Bringing in friction and air resistance introduces additional forces that only becloud the question. The question was, if the engine pulls ahead on the train and the train pulls equally back on the engine, how can there be motion? (Acceleration evidently is meant.) The answer is, there cannot be acceleration of the center of mass of the engine and train if the only forces are those just mentioned. If "the system as a whole" means engine and train, then the last statement of the quotation is easily seen to be incorrect.

> (*5*) *Newton's third law of motion*. . . . The push of the finger on the block is the action and the push of the block on the finger, the "reaction." The action and the reaction never act on the same body. If the push of the finger is greater than the force of friction between the block and the surface on which it is resting, something must happen so that the reaction of the block may continue to be equal to the action of the finger. What does happen is that the block receives an acceleration; this acceleration must obey the second law, $f = ma$. The total reaction on the block is consequently the sum of the frictional reaction and the kinetic reaction ma.

If the author of (*5*) had stopped with the first two sentences quoted it would have been better. The part of the paragraph that follows is not only quite unnecessary, but is confusing. After being told very definitely that the reaction is the push of the block on the finger, we read of the total reaction *on the block*, the frictional reaction and the kinetic reaction ma. It is not necessary for "something to happen" in the way of acceleration in order that the reaction of the block may continue to be equal to the action of the finger. It is true that if, as in the illustration given, the total force on the block is different from zero, there *will* be an acceleration; but such a result, which is in accordance with the second law, is quite independent of the third law. On the supposition that the block is in motion, the force of the finger on the block might well be more than, equal to, or less than the force of friction on the block, but the reaction of the block on the finger would always be equal to the push of the finger on the block. Whether the force of the finger on the block is more or less than some other force on the block is an entirely irrelevant matter.

> (*6*) Newton's Law of Reactions. . . . We are here concerned with what are known as *internal forces*. The pulls, pushes, gravitational forces, etc., that we have so far considered are spoken of as *external forces*, because they are always applied by some external agency. . . . A simple example will serve to make clear what is meant by a reaction. Imagine a weight of W pounds resting on a table. This weight is clearly exerting a pressure on the table. But if W were the *only* force acting, motion would ensue. The weight would go through the table. The only explanation of its not doing so is that this motion is resisted by *an equal and opposite force R*, exerted by the table upwards the instant W acts on the table downwards, and these two therefore balance. The force R is called a *reaction*, and is only exerted by the table when an external force W is applied. This is an example of mutual interaction which always occurs when an external force acts on a body.[3]

The author here seems to say that Newton's law applies only to internal forces, and that another external force is needed in order that such internal reaction may occur. It is not at all clear what is meant by the symbol W. First, W is represented as *resting* on the table, as if it were the mass or object lying there. Next, it is a force

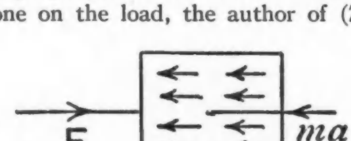
[3] Hart, *Introduction to physical science* (Clarendon Press, 1925).

acting, which would cause motion of the body and, therefore, evidently acts on the body. Finally it acts downward *on the table*. If W represents the pull of the earth on the body, then the upward push of the table on the body and W are not action and reaction, but are two forces of equal magnitude and opposite directions exerted by two different bodies on a third, thus keeping it in equilibrium. One pair of action and reaction consists in the force exerted on the table by the object, and the force exerted on the object by the table. These forces, of course, are equal in magnitude and opposite in direction, even if the body is going through the table, and have nothing to do with the equilibrium of anything. It may be stated that a very good example of a weight hanging on a hook immediately follows the paragraph quoted, but the good example can scarcely make amends for the one cited.

> (*7*) Newton's third law of motion. . . . The hand of a person sustaining a load is subjected to a downward force—the weight of the body—and the hand applies an equal upward force to the load. Similarly, a person applying a pull to a rope experiences an equal and opposite pull which the rope exerts on his hands. [Quite right so far.] Equal and opposite forces applied in the same straight line to a body balance one another; under such conditions the body, if at rest, remains at rest, or, if in motion, will experience no change in motion.
>
> A force applied to a body by means of some external agency, such as a pull along a string attached to the body, produces acceleration in accordance with the law $F=ma$. In this case the body, by virtue of its inertia, supplies a reaction equal and opposite to the force applied to it by the external agency. In Fig. [1], F is the external force applied to the body; each particle of the body contributes to the equal and opposite reaction by virtue of its inertia, and the total or resultant reaction is represented by the product ma. In fact, the equation $F=ma$ should be understood to represent the equality of two opposing forces, one, F, being the resultant external force applied to the body, and the other, ma, being an internal force produced by virtue of the inertia of the body.

After saying, properly, that one force is on the hand, and one on the load, the author of (*7*) immediately speaks of them as applied to *a body*, and confuses the condition with equilibrium. The second paragraph illustrates the erroneous idea that in the equation $F=ma$, ma is a force acting on the same body on which F acts, but in the opposite direction. The statements, and especially Fig. 1, make it perfectly plain that the author thinks a body can furnish a force ma on itself, which he looks on as the reaction to F. Actually ma is simply another expression for F in terms of the mass and acceleration of the body. It is not opposed to F, nor is it a reaction. It does not exist at all as a second force acting on the body. It is the same force as F. If there were such a "reaction" as that pictured, the student would be amply justified in asking how the body can be accelerated. Also, if such a picture were true we should have to write $F=-ma$. There is no such force acting on the body "by virtue of its inertia."

FIG. 1. Example (*7*).



(*8*) Owing to the extent of the material it is not convenient to quote from (*8*). In it a great deal of emphasis is placed on a "kinetic reaction" which evidently fills the same place as the "force of inertia" in (*7*), for it is used to denote $-m\dot{v}$ in the equation $F-m\dot{v}=0$. The author of (*8*), however, attributes $-m\dot{v}$, not to the body on which F acts, nor to any other body, but to the whole universe, or, as it is put in another place, to the ether. He asserts that $-m\dot{v}$ is not a force, as F is, but should rather be called an action. His definition for action seems to differ in no way from that of force, yet it is stated that the kinetic reaction $-m\dot{v}$ cannot be called a force without producing confusion. The author of this article believes that, considered as a force or an action on the body on which F acts, this kinetic reaction is wholly imaginary and nonexistent. The equation, $F-m\dot{v}=0$, which the author of (*8*) properly regards as a fundamental relation in mechanics, but interprets as stating that the sum of all the actions on a body is always zero, is easily seen, by adding equals to both sides, to be simply Newton's second law. If a body is to have acceleration, the sum of the "actions" on it must not be zero, for matter is inert, and needs some "action" to enable it to change its velocity. If anything is called kinetic reaction it should be the reaction mentioned in Newton's third law, which is on a different body, and therefore should not be com-

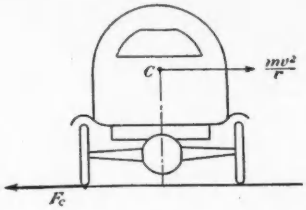


Fig. 2. Example (*10*).

bined with F to produce a null action on one body. In (*8*), as in some other texts, the kinetic reaction is said to put the body in "kinetic equilibrium." "Kinetic equilibrium" appears to cover every condition of a body that has acceleration, and hence means nonequilibrium. Kinetic equilibrium is an undesirable term. A body is either in equilibrium or it is not. It is not in equilibrium if $\Sigma F \neq 0$. The treatment of $-ma$ or $-m\dot{v}$ by the authors of (*7*) and (*8*) is quite analogous to writing $v-(ds/dt)=0$, and arguing that $-ds/dt$ is a sort of kinematic reaction, so that we can state that the sum of the motions which a body possesses is always zero, and the body is in a state of kinematic rest.

> (*9*) Newton's third law states that to every action there is an equal and opposite reaction. Action and reaction here mean an active force and an opposing force. When a mass rests on a table the active force is due to gravity acting upon the mass. . . . It often happens that a body moves when acted upon by a force. In this case, the active and reactive forces do not appear equal, and Newton's third law does not seem to apply. However, the applied force causing acceleration is just equal to the reacting force which resists acceleration, and Newton's third law applies.

Comment is hardly necessary here. The hesitancy of the author to apply it to a body experiencing acceleration gives the impression that only the acknowledged prestige of the law keeps him from doubting it altogether for bodies undergoing acceleration. Under the mistaken idea of regarding the law as an expression of the equilibrium of two forces, he is quite justified in his doubt, for if this idea were the true one the law would not apply to an accelerated body.

(*10*) The misconception of the reaction as a force acting on the body that is the object of the action sometimes leads to an erroneous treatment of centripetal force, as follows. Figure 2 is reproduced from the text in order to make the statements clear.

> Illustrations of central forces. . . . The reaction to the force which holds the car in a circular path acts through its center of mass, and combined with the force of friction F_c acting on the tread of the wheels, produces a couple tending to overturn it. This is shown in Fig. [2] where the car is supposed to be turning to the left, and the couple tends to overturn it to the right. That is, it tends to tip over *outside* the circle it is moving in.

From another part of the text we find that the author of the foregoing quotation holds that only a *couple* can exert a torque, and therefore he is obliged to introduce a nonexistent force on the body, mv^2/r, acting at, or through, its center of mass and away from the center of the circular path in order to tip the car over. However, a force acting at the center of mass would have no effect in rotating the body, but would only cause translation. The tipping of the car is the same whether this force acts or not. The force F_c, the larger part of which is applied to the other wheel, is the real mv^2/r, and is the only horizontal force acting on the car. It is the force F_c that causes the car to tip, if it does tip. If "overturn to the right" means that the center of mass moves to the right, that statement is also erroneous. As long as F_c acts, the center of mass moves to the left in Fig. 2, whether the car is tipping over or not. Moreover, if F_c remains equal to mv^2/r, the center of mass always remains directly over the circumference of the circle of radius r even if the car tips over; however, in event of such a catastrophe, v doubtless changes as well as F_c, and therefore the circle loses its significance.

If the forces indicated in Fig. 2 were really acting on the body, there would be nothing left to give the car the acceleration v^2/r toward the center of the circle, which is the distinguishing characteristic of motion in a curved path, and the car would continue in a straight line. The author thus pictures a distribution of forces that would make the assumed motion impossible. The reaction to mv^2/r, away from center of the circle, acts, of course, not on the car at all, but on the ground and hence has nothing to do with the motion of the car.

* * *

I wish to repeat, finally, that my purpose is not primarily to criticize textbooks, but to help, if possible, in rectifying errors that seem to me quite common in our teaching of physics.

Report on Physics Teaching Personnel—Spring, 1943

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THIS report has been prepared by the American Institute of Physics as part of a continuing effort to secure and make available factual information concerning various phases of the participation of physics and physicists in the war. It contains a tabulation of the data obtained from questionaries distributed to institutions of higher education in February and March, 1943. It supplements information obtained by the National Roster of Scientific and Specialized Personnel in December, 1942, a summary of which has been distributed to heads of physics departments.

I. SUMMARY

On the basis of returns from 794 institutions it is estimated that in the spring of 1943 there were not more than 4000 persons, exclusive of undergraduate assistants, engaged in the teaching of courses offered or sponsored by the physics departments of all institutions of higher education in the United States. The total physics teaching load carried by these individuals was estimated to be 58,330 clock-hr/wk, an average of 14.6 clock-hr/wk per individual. Approximately 71 percent of this load was devoted to regular college courses, 15 percent to ESMWT courses and 14 percent to other war-inspired courses. At least 76 percent of the estimated number of teaching hours and 72 percent of the individual teachers are in institutions approved for inspection and possible use by the armed forces in training programs that involve the teaching of physics.

One hundred and seventy-one institutions reported 482 new physics staff members appointed during the period from October 1, 1942 through about March 15, 1943; 231 of these were full-time appointments and 251 part-time. Ninety-seven percent of the full-time and 91 percent of the part-time appointments were made in institutions approved for inspection and possible use by the armed forces in training programs involving the teaching of physics. Only 14 percent of all new staff members came from the physics faculty or graduate schools of other collegiate institutions, 11 percent came from public and secondary schools, 53 percent came from other departments within the appointing institution, and the remaining 22 percent were drawn from sources outside of the teaching field.

Seventy-three institutions reported the existence of retraining programs for new staff members not experienced in teaching physics.

A shortage, based on known requirements, of 404 individual teachers was reported as of the time the questionaries were returned.

The apparent major disagreement between some of these figures—4000 persons teaching physics courses—and those presented in the Roster Bulletin No. 4[1]—2350 full-time physics teachers in December, 1942—disappear when the two are reduced to a common basis.

II. REPORTED DATA

The questionaries were sent in February and March, 1943 to heads of physics departments in 1373 institutions of higher education. These institutions were selected from the 1756 listed in the 1941–42 *Educational Directory*[2] by eliminating all institutions which because of their nature (law schools, for example) or for other reasons were known not to offer work in physics. The following information was requested:

1. Number of persons teaching physics courses, classified by rank, in the fall of 1940, 1941 and 1942 and in February, 1943.
2. Present shortage of teaching personnel based on *known* requirements.
3. Total clock-hours per week spent in contact with students in various types of courses (regular college courses, ESMWT, and so forth).
4. New staff members now teaching courses in physics: previous work of each, full- or part-time basis, date appointed, rank.
5. Efforts being made to anticipate possible future developments affecting the physics department.

[1] *The teaching personnel situation in physics*, War Manpower Commission, Bureau of Placement, National Roster of Scientific and Specialized Personnel, Bulletin No. 4 (Revised), June 15, 1943.

[2] *The educational directory of the U. S. Office of Education*, Part III, "Colleges and universities," 1941–42.

It was found convenient to divide the reporting institutions into two groups: (*i*) those certified by the Joint Committee for the Selection of Non-Federal Educational Institutions for inspection and possible armed forces contracts involving the teaching of physics courses, and (*ii*) those not so certified. These will be referred to hereafter as "certified" and "noncertified" institutions, respectively. It does not seem likely that all "certified" institutions will actually receive contracts from the armed forces for training programs, but practically all institutions eventually used for such programs will almost certainly be included in the "certified" list.

Table I gives an analysis of returns by type and class of institution; Tables II and III indicate the number of individuals teaching physics and the teaching loads reported. It will be noticed that the average number of persons teaching physics in the spring of 1943 per "certified" institution is 6.35, that the average number per "noncertified" institution is 1.25 and that 80 percent of the individuals reported are in "certified" institutions.

Combining the data of Tables II and III, we can compute an average weekly teaching load that will give some measure of the extent to which the individuals reported are engaged in full-time teaching. This figure comes out 12.1 contact-hr/wk per teacher in "noncertified" institutions, 15.4 contact-hr/wk per teacher in "certified" institutions and 14.7 contact-hr/wk per teacher in all institutions. The fact of the average weekly teaching load per individual being lower in "noncertified" institutions is taken to mean that there is a higher proportion of part-time teachers in such institutions than in "certified" institutions. The figures are not measures of the average teaching load of regular, full-time teachers.

A somewhat similar measure may be provided by computing the "equivalent full-time" teachers required to carry the reported teaching loads, assuming some more or less arbitrary standard for a "wartime normal" individual teaching load. Table IV shows the results of such a computation, using 20 contact-hr/wk as the "normal" load.

The ratio of the number of individuals teaching to the number of "equivalent full-time" teachers (or the ratio of the assumed "normal" load to the

TABLE I. Return by type of institution.

Type of institution	Sent out	Returned	Percentage returned
Colleges and universities	659	480	73
Professional and technological schools	53	36	68
Teachers' colleges	171	97	57
Normal schools	7	3	43
Junior colleges	410	162	40
Negro institutions	73	16	22
Total	1373	794	58
"Certified" institutions	463	350	76
"Noncertified" institutions	910	444	49

TABLE II. Number of individuals (including part-time personnel) teaching physics courses at various times in 794 reporting institutions.

Date	Total	"Certified" institutions	"Noncertified" institutions
Fall 1940	2436	1934	502
Fall 1941	2398	1886	512
Fall 1942	2434	1883	551
Spring 1943	2750	2194	556

TABLE III. Reported teaching load in contact hours per week in various types of programs in 794 institutions—spring, 1943.

Program	Total	Institutions "Certified"	"Noncertified"
Regular college courses	28,745	22,848	5,897
ESMWT	5,915	5,410	505
WTS(CAA)	1,343	1,028	315
Naval flight prep.	772	772	
AAF air crew	2,252	2,252	
AAF premeteorology	1,049	1,049	
Miscellaneous war	380	365	15
Total	40,456	33,724 (83.5% of total)	6,732 (16.5% of total)

TABLE IV. Number of "equivalent full-time" teachers required to carry the reported teaching loads in the 794 reporting institutions, assuming 20 contact-hr/wk to be a "wartime normal" individual teaching load—spring, 1943.

Program	Total	Institutions "Certified"	"Noncertified"
Regular college courses	1,437	1,142	295
ESMWT	296	271	25
WTS (CAA)	67	51	16
Naval flight prep.	39	39	
AAF air crew	113	113	
AAF premeteorology	52	52	
Miscellaneous war	19	18	1
Total	2,023	1,686	337

TABLE V. Teachers added to physics department staffs in 794 reporting institutions between October 1, 1942 and about March 1, 1943.

Previous activities of appointees	Full time: "Certified" institutions	Full time: "Noncertified" institutions	Full time: Total	Part time: "Certified" institutions	Part time: "Noncertified" institutions	Part time: Total	Total: "Certified" institutions	Total: "Noncertified" institutions	Total: Total	
At appointing institution										
Other science, math., engr.	18	0	18	77	7	84	95	7	102	257 (53%)
Miscellaneous (admin., ed., law, music, humanities, and unspecified)	24	1	25	93	0	93	117	1	118	
Graduate students, former students	20	0	20	17	0	17	37	0	37	
At other educational institutions										
College faculty (physics)	49	6	55	2	3	5	51	9	60	68 (14%)
College graduate students (physics)	8	0	8	0	0	0	8	0	8	
Secondary and public schools	39	0	39	14	2	16	53	2	55	
Elsewhere										
Not indicated	12	1	13	5	4	9	17	5	22	
Industry	12	0	12	3	1	4	15	1	16	
Government work	10	0	10	2	2	4	12	2	14	
Engineering	10	0	10	3	0	3	13	0	13	
Miscellaneous*	21	0	21	12	4	16	33	4	37	
Totals	223	8	231	228	23	251	451	31	482	

* "Miscellaneous" includes (totals): Radio, 6; research, 6; housewives, 5; aviation ground instructors, 4; retired professors, 2; contractors, 2; merchants, 2; minister, astronomer, lawyer, insurance representative, pharmacist, farmer, Army dischargee, 1 each.

actual load) may be considered as a maximum "expansion factor" indicating the upper limit to which teaching might be expanded without the use of additional teaching personnel. That it is an upper limit rather than a feasible expansion is obvious from a consideration of the following: (*i*) individuals are not necessarily distributed, and probably cannot be redistributed, in such a way as to permit expansion to a uniform teaching load; (*ii*) an undetermined number of persons contributing part-time services is not available, because of other employment or for other reasons, for full-time duty. These "expansion factors" are: for "noncertified" institutions, 1.66; for "certified" institutions, 1.30; for all institutions reporting, 1.36. Thus, for example, "certified" institutions could not expand their total teaching load by more than 30 percent with present teaching personnel even if an ideal distribution of teachers were possible and all part-time personnel were available for full-time duty.

Table V is a tabulation of appointments made between October 1, 1942, and the date of return of the questionary, and indicates the previous activity of the persons appointed. Most appointments made during the period reported may be considered "emergency" appointments, and it is believed that the data presented show with some accuracy the extent to which teachers were recruited from emergency sources. It will be noted that about 27 percent of the full-time and 2 percent of the part-time teachers appointed came from what might be considered normal sources—faculty members and graduate students of physics at institutions other than the one to which the appointments were made. That is, about three-fourths of the persons appointed to teach physics on a full-time basis were drawn from emergency sources.

A total "shortage" of 404 teachers was reported. "Certified" institutions accounted for 387 (96 percent) of these. It should be noted that this figure includes only teachers the need for whom was fairly definitely established at the time the questionary was returned, and therefore does not include the needs of part of the Air Forces programs, a considerable part of the Army Specialized Training Program or of any of the Navy V-12 program.

Because of the wide variety of comment made in response to the question about "efforts being made to anticipate possible future developments,"

no attempt was made to tabulate replies except to note that 73 institutions reported retraining activities for emergency teachers in progress or already completed. The general impression obtained from the response to this question was that, with few exceptions, physics departments appreciated fully the nature and extent of the problems facing them and were meeting these problems with a high degree of insight and resourcefulness.

III. ESTIMATED DATA

Making use of the percentage return figures, one can make estimates as to probable total figures for all institutions of higher education. Differences in the size of physics staffs make it desirable to correct separately the data for "certified" and "noncertified" institutions. Application of these corrections leads to the data in Tables VI and VII, corresponding to Tables III and IV of the previous section.

This process of extrapolation gives for the number of individuals teaching physics in the spring of 1943 in all institutions of higher education an estimated figure of approximately 4000. Of these, 2875 are in "certified" institutions and 1125 in "noncertified" institutions. These numbers are not directly comparable with those[3] in the National Roster's Bulletin No. 4 (Revised)[1] since the former include part-time teachers whereas the latter are for full-time personnel only. The data of Table VII are more nearly comparable, although not precisely so since the data were taken at different times, the bases for computing full-time equivalents may differ and the extent to which programs outside the regular college courses are represented in the Roster's figures is not indicated in the report.

It will be seen, however, that the Roster figure of 2350 full-time teachers in all institutions lies between the 2115 equivalent full-time teachers involved in regular college courses and the 2623 involved in all work exclusive of the armed forces training programs. It is believed that this constitutes as good an agreement between the two sets of figures as can be expected in view of the factors noted above.

TABLE VI. Estimated teaching load in contact hours per week in various types of programs in all institutions of higher education—Spring, 1943.*

Program	Institutions: Total	"Certified"	"Noncertified"
Regular college courses	42,300	30,100	12,200
ESMWT	8,170	7,140	1,030
WTS (CAA)	1,990	1,350	640
Armed forces training programs and miscellaneous	5,870	5,840	30
Total	58,330	44,430	13,900

* These estimates probably are somewhat too large since it is likely that the nonreporting institutions to which the reported data are extended are less active in physics than the reporting institutions.

TABLE VII. Estimated number of "equivalent full-time" teachers required to carry the estimated teaching loads, assuming 20 contact-hr/wk to be a "wartime normal" individual teaching load—Spring, 1943.*

Program	Institutions: Total	"Certified"	"Noncertified"
Regular college courses	2115	1505	610
ESMWT	408	357	51
WTS (CAA)	100	68	32
Armed forces training programs and miscellaneous	294	292	2
Total	2917	2222	695

* See footnote to Table VI.

[3] The Roster survey, made in December, 1942, lists 2350 full-time physics teachers (more properly, full-time-equivalent) in all institutions of higher education. Of these, 1700 were in "certified" institutions and the remaining 650 in "noncertified" institutions.

EVERY application of science to a useful purpose is, so to speak, a dividend paid out of surplus. In lean times and to meet emergencies it is possible to continue, at least for a time, to declare dividends from surplus. But in the long run the surplus will not be there unless basic earnings are steadily accumulated. And the basic earnings of science come from pure research.—WARREN WEAVER.

On Existence and Complementarity in Physics

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I

THE present situation in physics has given rise to the question whether or not the quantum mechanical theories do represent "reality." This problem has been treated extensively in the literature. But most arguments used to refute the thesis that modern physics deals with a fictitious world are concerned with the more technical aspect of this question: It is usually the mathematical symbolism that is under discussion. Moreover, new arguments have been adduced which demand a reply. In the line of reasoning adopted here it will be attempted to treat the problem from the philosophical, or logical, point of view which has been neglected previously.

Now, one may argue that it need not trouble the physicist—however much it might be of concern to the philosopher—whether or not a theory furnishes a picture of reality; that is to say, whether the symbols employed in the theory "correspond to an element in reality." The success of a theory alone is important. The theory has to provide an adequate description and prediction of experience; then the theory is verified. Thus Dirac says that "only questions about results of experiments have a real significance and it is only such questions that theoretical physics has to consider."

Of course, this is an unsatisfactory way of stating the problem. Certainly a symbol has to refer to something. This merely expresses that it must be applicable under specified conditions: Then it has a meaning. If the symbol is used within a theory, and if the theory is confirmed by experience, then the whole theory describes a real state of affairs. Only in this manner can symbols be said to refer to reality.

We need not decide here upon the usefulness of the term "existence" in physics. Yet we are justified in asking whether we employ the term in quantum physics in the same sense as in classical physics or in daily life. For it is from this source that arise the arguments about existence. It may be granted that we speak of existence legitimately when we describe the familiar things of everyday experience; we speak of the existence of trees and tables and chairs. However, already in the sphere of daily life we encounter difficulties regarding existence. The simple "argument from illusion" demonstrates the truth of the statement that *percipi non est esse.* An equally simple argument—for instance, about the other side of the moon—proves the converse to be true also. Psychologically, the concept of existence is derived from daily experience. But the "subjective" existence of our psychological experience is a vague concept; it is insufficient to describe the whole of our experience. We have to submit the term to a logical analysis in order to clarify it; for we must be able to distinguish between dreaming and being awake. When we meet with "abnormal" experiences we realize that the naïve interpretation does not hold. This is equally the case in physics.

The situation concerning "existence" in quantum physics appears to resemble the position taken by relativity theory with regard to time. Existence—within a scientific theory—is not absolute or self-evident and immediately given. Sense perception and observation alone cannot decide the question. Existence is not a property or a predicate, it merely *describes* a certain state of affairs; this state of affairs has to be analyzed. Thus existence is a term that has to be given by a coordinating definition. The definition has to be formulated in such a way as to permit the application of the term in experience. Moreover, we prefer to use the term in agreement with previous experience; we do not wish to offend our psychological experience of daily life. We must then state what definition is actually in use, and find a "good" definition. This problem, I believe, is solved by modern physics.

The existence of the elementary particles in physics has often been discussed. Protons, neutrons and electrons have been demonstrated to exist by means of experimental procedures that do not deviate too much from the methods we employ in daily life when we speak of the exist-

ence of an object. For example, electrons and protons are shown to exist by tracks on photographic plates exposed in a Wilson chamber. Thus these particles can be said to have been made "visible"; we can see them nearly as well as chairs or tables. In these cases we seem to use the term "existence" in the same sense as in daily experience.

Recently, the existence of another particle—the neutrino—has been postulated. The evidence is based upon experiments concerning the beta-particle decay of certain radioactive substances. The particle is devoid of electric charge, and it possesses a very small mass—perhaps none at all. Therefore, its existence cannot be ascertained by "direct" means; it is required to exist, however, if we want to retain the principle of conservation of energy. This principle is a basic assumption not only of classical but also of quantum physics, and thus the existence of the neutrino has to be and has been accepted by all physicists. The whole system of science would have to be revised otherwise.

We can then state a sufficient condition for the existence of a physical entity: It must be in accord with the laws and principles of physics. That the converse holds true is shown by the case of the ether. The ether has been assumed to exist since the observable *actio in distans* was deemed to be unthinkable. But the ether could not be found to behave according to the physical laws; this was proved by the experiments of Michelson and Morley.

Of course, imagined phenomena might also appear to follow the laws of physics. Indeed, most of the perceptual data occurring in dreams are normally of the same kind as those we experience when we are awake. Only sense perception can provide a basis for propositions to be incorporated into the system of science as we know it today. But the propositions must be capable of being tested by experience with the help of accepted scientific methods; and they must enable us to predict future experiences. Only experience can furnish a *necessary* condition for the truth of any empirical proposition, or for the existence of a physical fact; a *sufficient* condition is given by the agreement with previously established knowledge.

We might say then that "x exists" or "x is real" or "this event or state of affairs is real" means: "x has been found by experience to follow the laws of physics" or "this event has been observed, and it can be described exclusively by the help of physical laws." In this manner we formulate the necessary and sufficient condition for the existence of a physical object; and it is the same condition that underlies our usage of the term "existence" in everyday life as well as in classical physics. We shall see that the same condition also holds for quantum physics—with one difference, however: The quantum laws are of a kind different from the classical laws.

II

Some time ago Einstein[1] proposed that "a sufficient condition for the reality of a physical quantity is the possibility of predicting it with certainty, without disturbing the system." The theory should not only be correct (that is, the results predicted by the theory should be in numerical agreement with the experimental findings), but the description given by the theory should be "complete." From the logical point of view it is interesting to note that completeness of description is demanded here as a condition of existence.

In the simplest sense complete description of physical phenomena is never possible. What we know of electrons today is not all we will ever know; we will find out more about them tomorrow. This is a trivial statement, of course. What Einstein wished to postulate is the condition that an electron, for instance, should be completely describable by the two symbols p and q, which represent the generalized coordinates of momentum and of position in classical particle mechanics. This, however, is the case as Bohr pointed out.[2] The p's and q's can be determined completely—that is, they can be measured singly with unlimited accuracy—but they cannot be measured simultaneously without introducing an error. Two alternative descriptions are offered which are complementary to each other. The "incompleteness" appears only when we wish to combine the two descriptions; or, rather, when we measure the properties of an electron from

[1] Einstein, Podolsky and Rosen, Phys. Rev. **47**, 777 (1935).

[2] N. Bohr, Phys. Rev. **48**, 696 (1935).

two different points of view; or, when we measure two different properties simultaneously. It is true that in the realm of classical mechanics we can obtain a simultaneous measurement in these two ways. But in classical physics idealized conditions are always assumed. Observation and measurement are operations that it must be physically possible to carry out. We are forced to acknowledge that the measuring device must needs influence the phenomenon to be measured. Without interaction no observation would be possible, for our measuring instruments have to react with the phenomena. The interaction between them can be neglected in macroscopic physics—but in quantum physics it has to be taken into account, because of the finite magnitude of the quantum of action.

The quantum mechanical description, therefore, is more complete since it considers this interaction which we disregard in classical physics due to the gross approximation with which we are describing the system. There is no good reason to demand that this accidental feature of a "rough" description should be maintained when we deal with a closer approximation, a more detailed description, as we do in quantum physics. A scientific description can be furnished only by observation, by measurement; it cannot be more comprehensive than the measurement, it cannot demand more data than nature is willing to cede to us. Otherwise we seem to withdraw the empirical basis upon which all physics rest.

To ask for a "complete" description, then, implies that the quantum phenomena should be subject to the laws of classical physics and that they should lend themselves, thus, to visualization by means of simple mechanical models, as it had been proposed once by Lord Kelvin. This is emphasized in a recent paper by Einstein. While admitting that "all attempts to represent the particle and wave features displayed in the phenomena of light and matter, by direct recourse to a space-time model, have so far ended in failure" and "that any decision as to a rigorously deterministic structure is definitely ruled out," nevertheless, he "cannot believe that we must abandon, actually and forever, the idea of direct representation of physical reality in space and time; or that we must accept the view that events in nature are analogous to a game of chance."[3]

Indeed, we have arrived at a stage in physics where it is no longer possible to interpret the symbolism by means of the familiar pictures we are confronted with in daily experience. This aspect of quantum physics has often been stressed, particularly by Heisenberg and by Dirac. However, most of our concepts have been derived in this way; as soon as these concepts become more formalized and are incorporated into a scientific theory, their meaning appears to change. Only a partial meaning, namely, that part which we can grasp by simple visualization, seems important to us; for what is psychologically primitive, is often logically complex—and what is logically clear, is often psychologically obscure and offers great difficulties in understanding. In every day life we do not separate the psychological and the logical element in our knowledge. One forgets that the psychological import of a term and its logical range may coincide more or less within common sense but differ within a more advanced theory; that its psychological meaning—associations, images, and so forth—is different from its logical meaning—that is, from what it can stand for in a proposition. Our "intuitive" understanding does not suffice for it does not restrain us from errors. But it is this psychological element that we associate with the term in ordinary language. Therefore, its interpretability in every day language is different from its logical definition.

The importance of a picture does not consist in its appeal to the optical sense but in the logical relations it exhibits. Any abstract representation in logical or mathematical terms can describe the facts equally well: Even the most vivid picture is still a picture—a symbol that resembles the fact but does not coincide with the fact it depicts. Our intuitive understanding may guide us—sometimes mistakenly—to obtain the logical meaning; logically, the psychological associations are entirely irrelevant. Immediate psychological experience may be the basis of even the most formalized concept. Yet only in the world of human dimensions can we expect to find situations that can be visualized by the simple picture which represents our familiar world. At very

[3] Einstein, Science **91**, 487 (1940).

small or at very large dimensions this accidental visual representation cannot hold any longer. Thus the symbolic character of all representation becomes apparent, and only the logical meaning of the concept and not the psychological understanding is important. Symbolism, however, does not allow of an imagery in most cases. Scientific symbolism shuns visual, pictorial representation of what the symbol stands for; it is abstract.

To deny existence to phenomena which follow the laws of quantum physics but not those of classical physics, thus appears as an attitude difficult to share. The only advantage of classical physics consists in its utilizing concepts that appear to be simple and self-evident because they stem directly from our daily experience. Laboratory experience has compelled us to adopt a more detailed description of nature which revealed different and new laws of physics. These laws are "extensions" of the classical laws justified by the correspondence principle of Bohr. They are based upon and tested by experience—a more detailed and more exact experience than in classical physics. To deny existence to phenomena described by quantum mechanics and to demand a "direct" representation is tantamount to a refusal to accept quantum physics as a whole.

Naturally, it is always possible that our theories may be wrong. It is conceivable that future experience might force us to revise radically the quantum mechanical theories. All scientific theories are approximative, after all; and the introduction of classical concepts into quantum physics—the conservation principles, for instance—creates difficulties which will have to be eliminated. But it is not very likely that fundamental facts as they are represented by the uncertainty relations will disappear; the success of the theory, so far, has been too great. Moreover, just because it has brought into prominence the concept of probability, our belief as to the validity of the theory is strengthened: for it appears to reflect the fundamental character of all scientific knowledge.

III

The objections to quantum mechanics seem, then, to derive from the reluctance to accept probability as a basic element of knowledge. The indeterminacy relations demonstrate the logical impossibility of measuring conjugate quantities with unlimited accuracy; hence it is felt that the theory is at fault. Or, that the limitations inherent in any measurement are imposed upon it by the theory—providing the conceptual frame for the experiment—and not by the nature of experience itself. The assertion is made that because of inadequate concepts we obtain results which give probable rather than absolutely certain values as furnished by classical physics.

These objections involve two serious misconceptions, I believe. First, it is supposed that facts are moulded by the language used to describe them. The assumption is made that facts can be changed by the manner in which we speak about them; and that, thus, an inadequate linguistic expression of a fact may entail a misrepresentation of the fact. The second assumption implies that there are measuring technics—namely, those of classical physics—which can yield absolutely certain results.

Now, it cannot be said that any measurement can ever give certain results, even though the operations may not be subject to any restriction. For measurement is a physical process and, thus, rests ultimately upon the occurrence of sense perception. Moreover, measurement always signifies a prediction: It is good only if it can be shown to be valid at future occasions. All propositions about physical laws or about physical phenomena are probability statements since they involve inductive reasoning. This general epistemological result of scientific philosophy has to be distinguished from the particular empirical result of quantum physics. We prefer to describe the physical world by the help of statistical laws for they alone can account not only for the behavior of quantum phenomena but also for that of the classical phenomena; the deterministic laws of classical physics are a special case of the statistical laws—the latter are more comprehensive. In quantum physics, the initial conditions of a physical phenomenon are not exactly known because of the uncertainty relations; it is a logical impossibility when we realize that measurement is a physical process, and it is not a question which would depend on any alleged imperfection of the observer or of the instrument. The quantum mechanical theories show why the

interaction of the measuring device with the measured phenomenon can be neglected in classical physics. But even if we were perfect observers and possessed perfect instruments, we still would have to contend with the fact that empirical knowledge is inferred and hypothetical; even deterministic laws can be formulated, in this sense, only as probability statements since they are obtained by the help of induction. Quantum physics has shown that certain phenomena follow statistical rather than deterministic laws—we have here, so to speak, merely a change in the type of law: but all statements about any physical law are probability statements. It is a fallacy to believe that the quantum mechanical methods yield less reliable results than the classical methods and that, for this reason, they have to be abandoned. Although classical physics seemingly enables us to predict the result of future experiences with absolute certainty, the result itself is afflicted by the fundamental uncertainty of all empirical knowledge. Ever since Hume it has been known that a proposition about empirical matters of fact can never be absolutely certain.

It is presumably correct to say that the language we happen to use is a matter of convenience and convention. An inadequate scientific language may not be too successful in describing the physical world. But this does not mean that there appears an essential restriction which can be ascribed to the theory. The verbal expression of a physical event does not influence the extraverbal occurrence of this event. A theory—that is, the linguistic calculus describing physical events and their interrelations—does not change the course of the universe. Neither the instruments by means of which we acquire knowledge nor the theory which represents the linguistic formulation of this knowledge can ever change the physical world. It is a strange assertion that "it is the inexorable law of our acquaintance with the external world that that which is presented for knowing is transformed in the process of knowing."[4] For we do not acquire our knowledge by intuition but by experience; we cannot uphold the idealist's postulate of a world behind the world which we experience.

The field of experience is not limited by the theory in the sense that the theory may misrepresent the facts. Experience can be limited only by experience; and the guaranty is given by the condition that we verify our propositions and that we allow "observables" only. Classical physics certainly employs an inadequate concept in assuming time to be absolute; and yet it cannot be said that classical physics is false. It merely is unsuccessful in accounting for certain experiments which relativity theory can describe by including a revised concept of time. If we are able at all to test by experience the propositions expressed in a linguistic system, then we may have a poor or clumsy representation—it may be inadequate in this sense; but it is not a representation which is restricted in such a way that it actually falsifies experience. Although the rules of scientific language may be arbitrary to some extent, the propositions expressed are not arbitrary but are based on experience. Thus a linguistic calculus may or may not be adequate according to the ease with which it permits to construct propositions. *How* a proposition is expressed naturally depends upon the language; but not *what* it expresses—this is given by experience.

Therefore, the Heisenberg relations represent "a fundamental trait of nature." They do describe a fact; only their peculiar linguistic expression obviously depends upon the calculus we use to describe this fact. A different syntax would furnish a different symbolic description; the fact it describes still remains a fact. Since the classical concepts are inadequate, for it is difficult to interpret them in terms of probability, we might feel encouraged to develop a language with a different logical syntax which would permit a more adequate description of the world.

IV

The dualism which has shown up in quantum physics due to the use of classical concepts, suggests an investigation of these concepts. This is mainly a psychological problem; but it also is a logical one for these concepts are never explicitly and completely defined. Concepts, first of all, are words—"key words" functioning in a certain way within a physical theory. I do not

[4] A. Eddington, *New pathways in science* (New York, 1935).

mean here by the term *concept* the personal, psychological associations and sensations that enable the physicist to visualize a physical situation. I mean the concepts as they occur in an established scientific theory: the definitions or postulates in terms of which statements of the theory can be formulated. *Mass*, *energy*, *wave* may serve as examples. These, too, have a social and psychological meaning attached to them. As words taken from ordinary language they seemingly derive their meaning from the images associated with them. Thus we can grasp their meaning, we understand psychologically what is meant through the associations; we understand the terms without knowing them. For this reason it may be interesting to trace the psychological origin of our "primitive concepts." The notion of complementarity introduced by Bohr casts new light upon an investigation of this kind.

For many years it has disturbed physicists that light can be described either as a corpuscle (photon) or as a wave. The same holds true of "matter," that is, of the elementary particles, as has been proved by the experiments which gave rise to quantum mechanics. Hence the question arises: Which is real, the corpuscle or the wave; does the electron exist as a corpuscle or as a wave? A feeling of psychological discomfort is created; a contradiction is apparent: Something cannot be both A and B.

The answer ordinarily given is that the electron, for instance, is both—but not simultaneously. There are two equivalent theories, each describing one particular feature; and they are complementary descriptions arrived at by experimental procedures that mutually exclude each other. A similar situation exists regarding the p's and q's which describe the electron as a corpuscle; they are mutually exclusive, and together they are taken to represent in space-time all the properties of the electron.

It is said that they represent dual aspects; this is a very misleading expression. At once, one is tempted to ask: of what? The question leads to the metaphysical statement which some writers have made that wave and corpuscle are manifestations of one and the same underlying "real" reality. It is not even permissible to say that they are aspects in the same sense in which hardness, color and shape can be spoken of as aspects of a table. The logical character of these concepts is more involved.

First of all, since the waves in wave mechanics —the ψ function—have to be interpreted as "probability waves," it is asserted that, therefore, they are not real; they are "ghost" waves, they are only "symbolic" for they permit only probable predictions as to the space-time phenomena they represent. However, the ψ waves are real enough to produce observable phenomena—diffraction patterns—which cannot be accounted for otherwise. In the same way, corpuscles should be regarded as unreal then, for they are represented as diffused through space; they are not distinct and they have lost their individuality. Logically, therefore, corpuscles and waves in quantum mechanics are on the same level; they are equally "symbolic" when we compare them to the same concepts in classical physics. These concepts *are* the same words; the *same* symbols are used. Yet the usual psychological meaning can no longer be associated with them. They are more formalized—they are similar to the classical terms although they differ in some respects. Waves and corpuscles are quantum mechanical concepts which are analogous to those in classical physics but they are not the same concepts.

The wave concept, for example, has been increasingly refined by being constantly re-defined. From elastic waves in a medium as observed in simple experiments, a different concept of electric wave was developed which did not require a medium of which to predicate undulation. Now a still more abstract term—the probability wave—has found its application. The corpuscles are no longer miniature billiard balls such as are the classical particles; and the ψ wave is not the simple vibrational phenomenon of the classical wave. The classical terms carry a surplus meaning which narrows down their applicability; therefore, they are inadequate in quantum mechanics. Our immediate psychological experience provides associations that have to be abandoned; only the logical character, the mathematical expression, is important. We can find here an illustration of Russell's phrase that "the use of the word comes first, and the meaning is to be distilled out of it by observation and by analysis."

It may be somewhat gratuitous to speculate as to how exactly language affects our thoughts. But linguistic usage of a term—often arising from accidental or even mistaken previous application—has created pseudo-problems more than once; and it has been shown that the rules of grammatical syntax influence our use of a term. Historically, grammar is a rudimentary bivalent logic. This logic is the simplest type to invent as it is based on the most primitive kind of differentiation: the dichotomy. The problem of mind and matter is a stock example, and so are the "twin" words of which Russell once spoke. *Mind* and *matter* are usually taken as contradictories: in this way the question is already answered. But philosophers wrote lengthy treatises to prove that one was the other; they merely succeeded in demonstrating that the definition they used was accidental. The concepts employed in ordinary language are vague, and the psychological associations do not necessarily give the concept a logical meaning: They do not exhaust the possibilities of its application.

The dualism in modern physics then appears to be related to the syntax of the language we use. For the two descriptions are complementary in the sense that they mutually exclude each other, and they are assumed implicitly to exhaust jointly the universe of discourse. It is difficult to conceive of "something" as other than a corpuscle or a wave. The one concept stresses the discrete character while the other is endowed with continuity; and many similar psychological associations could be cited.

The complementarity of classical concepts is then caused by their implicit but accidental definition. In a scientific language we require a logical syntax more rigorous than the grammar of ordinary speech. The mathematical symbolism helps us over this difficulty. For we have two different mathematical technics which are based upon the application of two classical concepts; but the result of both theories is the same, and the only valid interpretation for both is furnished in terms of probability.

The situation is quite different from that found in relativity theory; among the many possible geometries there is only one that fits the physical world. In quantum physics we have two possible descriptions due to the introduction of "alien" classical concepts; but probability emerges as the basic and unifying principle. It is not so surprising that the higher degree in formalization in quantum physics necessitated by a more detailed experience should require a calculus with a more strictly logical syntax, and perhaps a different logic altogether—a three-valued logic or a probability logic. Thus it will not be possible to make use of concepts that can be visualized in simple pictures, for they are interpreted by the help of ordinary logic. We have to forego the familiar picture and to rely on an abstract, a logical picture. Such a representation can be made to describe the world, and to ask whether the ψ function, for instance, is a wave or not will be meaningless. This is exactly the position we are confronted with today in quantum physics.

The National Physics Building.—The national physics building acquired in September and now being paid for by contributions of physicists and their friends is already proving to be an influence toward professional solidarity. In one university physics department two or three individuals organized themselves into a canvassing team to solicit the support of their instructor and graduate student colleagues for the Fund. This effort has not only resulted in a large number of contributors but has led, in the words of one of the group, "to a large increase of interest on the part of graduate students in their work, and a growing list of applications for membership in one of the societies."

Outline of an Empiricist Philosophy of Physics (Concluded)

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IV. THEORIES

EVEN an outline as selective and nontechnical as this must at some place indicate the role which logic and mathematics play within the methodological schema of science.[19] The sharp dichotomy between logical and mathematical knowledge on the one hand and factual knowledge on the other is one of the cornerstones of any empiricist philosophy. 'Formal' and 'empirical' are simply two other names for the same pair of opposites. It is fair to say that the tremendous sophistication with which ever more precise formulations of this distinction have been elaborated constitutes one of the main achievements of the empiricist thought movement; it is mainly because of the services which it can render there that formal logic has become so essential for the new theory of knowledge.[20] Even the most complex chains of deductive reasoning, mathematical or otherwise, are as empty of factual content, and therefore as certain, as '2=2.' They are all in the same boat with the old standby, 'If (all men are mortal) and if (Socrates is a man) then (Socrates is mortal).' Clearly the three statements in parentheses are empirical, since their truth or falsehood depends upon facts; the compound statement, however, is true no matter what the facts are, and its truth is therefore formal. This is so because, *first*, the conclusion merely states (part of) what is stated in the premises, and, *second*, the compound statement as a whole asserts merely that the conclusion is true *if* the premises are true. It might help to avoid misunderstanding if it is emphasized that only mathematical identities are formal truths in this sense. The functional connection asserted in a physical formula—for instance, in the Newtonian gravitational formula, is not a mathematical identity, but the partial expression[21] of a quantified empirical law. It is, therefore, on the same level with 'All men are mortal.' If one wants to carry the analogy further, one might compare any set of initial conditions (state) of the planetary system with 'Socrates is a man' and, finally, Kepler's laws or, for that matter, any astronomical prediction within the planetary system with 'Socrates is mortal.'

All this is pitifully primitive and inaccurate, but my purpose is merely to recall certain general ideas which, on the one hand, are presumed to be known in some manner and, on the other, cannot possibly be presented here with even a small part of the sophistication to which they are amenable. The obvious and important point is that mathematical deduction merely makes explicit what is already contained in the premises and that any attempt to derive empirical laws from pure mathematics is, therefore, hopeless. But even a remark as plain and obvious as this does not set up straw men, for the group theoretical speculations in Eddington's most recent book are in the last analysis nothing but the latest product of those age-old Platonistic tendencies which try to derive factual from formal knowledge.[22] It seems

[19] In order to emphasize these remarks they have been placed at the beginning of a section. Otherwise, it will soon be noted, the transition is as gradual as that between the two preceding sections [Am. J. Phys. **11**, 248 (1943)]. A relatively nontechnical presentation of the main problems will be found in Carnap's *Foundations of logic and mathematics* (monograph I, 3 of the International Encyclopedia of Unified Science). The standard works for everybody whose main interest is not the formalism itself are Carnap's *Logical syntax of language* (1934, 1937) and his *Introduction to semantics* (1942).

[20] In **Sec. I** I mentioned the syntactical phase of empiricism. This can now be somewhat elucidated. The first, most important, and most difficult achievement was the recognition of logic and mathematics as an essentially unified formal structure without factual content or, as one also says, as *syntactical* truth, tautological and thus *a priori*. There was therefore a tendency to press every statement which was rightly felt to be nonfactual into the Procrustean bed of syntax. With the newer methods, however, one can also elucidate the formal elements in the relations among languages, their referents and their users (semantics and pragmatics). The concept 'formal' is therefore, in a certain sense, broader than the concept 'syntactical'; and this broadened frame of reference allows for a much more sophisticated dissolution and transformation of the old metaphysical issues without, however, diluting the essential thesis that all knowledge which is not empirical in the common-sense meaning of the term belongs to the linguistic structure and is, like the camera, never part of what it depicts.

[21] The other part consists of the operational definitions of the constructs which occur in the law. Concerning the way in which the quantification is achieved, see footnote 9.

[22] For a remarkable analysis of Eddington's *The philosophy of physical science* see Braithwaite's review in Mind **49**, 455 (1940).

significant to me that the same writer, in an earlier stage of his thought, was given to overphysicalistic formulations. This has been pointed out before.

We are now prepared to understand the basic connotation of the term 'theory.' Consider, for instance, the three formulas connected with the description of a free falling body:

$$s=\tfrac{1}{2}gt^2, \quad v=gt, \quad a=g.$$

They contain one parameter, g, and, besides, four operationally defined constructs, s, t, v, a, that are not independent of one another. First, these constructs rest upon the same operational basis; this almost self-explanatory expression signifies that the situations to which they apply are either the same or contain at least many common elements. Second, their definitions themselves contain, in a certain order, each other; in less simple cases they contain common clauses. The point now is this: Obviously none of the three empirical laws can be deduced from the definitions. This would be as absurd as trying to derive them from mathematics.[23] However, experimental evidence for any one of the three laws is *ipso facto* also evidence for the other two since they are logico-mathematical consequences of one another *and* of the definitions that interrelate the variables. (I neglect the refinement necessitated by the constants of integration, which is both obvious and irrelevant for our purposes.) Thus a deductive connection has been established among several empirical laws. Such organization of empirical laws into deductive systems is the distinguishing characteristic of scientific theories. There are, as we shall see, different technics which lead to deductive integration; but if one had to be selected as the prototype for all of them, I should choose the case where the deduction of empirical laws from other empirical laws is achieved by using, as additional premises, those relations among the occurring variables that are given in their definitions. This is indeed the case that our elementary example illustrates. Another deductive technic, the derivation of new laws by elimination (in our example, $v^2=2sg$) is methodologically rather trivial.

[23] This similarity reflects itself in an older formulation according to which formal truth is "truth by definition." The phrase is still current, but one had better avoid it since it leads easily to confusion.

In order to avoid misunderstanding, it must be said that the isolation of such types of theory structure is essentially a descriptive job, not dissimilar in character to connotational analysis, and that actual scientific theories must therefore not be expected to be true to such pure types. But it seems to me that there are, besides the two elementary ones just mentioned, only two more relatively clear-cut deductive technics. These are represented by the electromagnetic theory of light and by atomic (model) theories, respectively. Since their analysis is less obvious, I shall, in the next two paragraphs, preface it by a few elementary comments and a survey of some of the more superficial connotations of the term 'theory.' But first of all I should like to express a terminological preference. It seems to me that communication is simplified if the term 'theory' is introduced as late as possible into a methodological discussion of the kind in which we are engaged. As long as one stays unambiguously within the realm of empirical laws and constructs, it might be as well to say so. I, for one, should therefore prefer to speak of systems of empirical laws, or of scientific systems, and reserve the word 'theory' for existential models (for example, atomic theories), since it is only this latter most sophisticated device for deductive organization that requires an additional methodological clarification. However, this proposal deviates from the common usage which is well established.

Theories are said to *explain* rather than merely to *describe;* to *unify* science; and to produce *understanding*. While it would be worth while to devote a monograph to a complete connotational analysis of these and similar terms, we shall here content ourselves with a few hints. In what sense, for instance, have Kepler's laws been unified by Newtonian mechanics, so that the latter explains what the former merely describe? Let us assume that Kepler had found the exponents in his third law to be 1 and 2, instead of 2 and 3, respectively. This result would not have disturbed him at all; as a matter of fact it might have pleased him even more, since he was still under the influence of Pythagorean-Platonic number mysticism and might have thought the smaller integers to be more beautiful. The success of Newton's inverse-square hypothesis, on the other hand, was predicated upon these constants being what they

actually are. Elaboration of these elementary points seems superfluous. Let me emphasize, instead, that what has just been called Newton's hypothesis was a candidate for a position as an empirical law even at the time he formulated it. This status of a statement does not depend upon the practical possibility and the directness of its verification, but merely upon its form, namely, that only empirical constructs occur in it. In this sense the attraction formula is, at least for terrestrial bodies, undoubtedly an empirical law. So one sees how the attempt at deductive organization might even lead to the formulation of empirical laws that are basic in the prospective system, or, as one usually says, theory. If, on the other hand, a deductive organization has been achieved, then it is as a rule possible to derive from it and subject to experimental test further empirical laws which stand in exactly the same relation to the basic laws of the theory as do the theorems of geometry to its axioms. However, if the terms 'basic' or 'axiomatic' are used with reference to empirical laws, it must always be understood that these characteristics refer to their position within a deductive schema. In itself no empirical law is more basic or fundamental than any other. Summarizing these remarks one might say that 'explanation' and 'description' are relative terms which refer to the positions of laws (and individual facts) within a deductive system. Another rather neat way of putting things is to say that while laws predict individual facts, theories enable us to predict laws. But this formulation is certainly oversimplified and as vague as the customary use of the word 'theory' itself. For a set of theoretically unrelated empirical laws or even a single empirical law might very well exhibit a predictive power which leads to its being dubbed with the honorific label of theory. The root of such predictive or explanatory power lies typically in the fact that the law or laws in question contain parameters which allow for their application to a variety of situations of widely diverging appearance and outcome.

Concerning the term 'unification,' it should be clear without further comment in what sense Newtonian mechanics has unified astronomy, but it is also commonly accepted that one of its most outstanding achievements lies in the unification which it has brought about between the two hitherto unrelated areas of mechanics and astronomy. Again, we leave it to an elementary textbook to show in what way and within which limits the word 'area' can be given a precise meaning. Obviously the concept of operational basis which we have introduced before will play an important role in any such attempt. As far as unification is concerned, there is no doubt that whenever it has been achieved between two or several previously unconnected fields or areas, understanding is felt to have made progress of prime importance.

Some doubt may have arisen as to how the deductive character of theories, which I have so strongly emphasized, can be reconciled with the patent fact that theories are daily being subjected to experimental verification, that it is even the common fate of theories to undergo modification and to be ultimately discarded because of the unsatisfactory outcome of such tests. So strongly have scientists been impressed with this very feature that some of them like to say theories are neither true nor false, but merely convenient fictions, or at least still one more step further removed from conclusive verification than empirical laws. This is, after all, the so-called instrumental character of theories which pragmatists are so inclined to stress. How does this viewpoint agree with our insistence on theories being deductive, that is, as one might think, true *a priori*, independent of fact? As far as the idea of convenient fictions is concerned, its analysis will have to wait until we discuss existential models. For the rest, however, the apparent contradiction is merely verbal; the whole thing has been brought up not as a real issue but merely as a pretext for the last one in this string of preliminary remarks about the nature of theory in general. Take the typical case where an empirical law, previously unknown or unnoticed, has first been deduced from a theory and then not been borne out by subsequent systematic experimentation. What we do in this case is, again typically, either try another theoretical organization, or redefine our constructs, or, more frequently, both. The typology of the situation is somewhat similar to, and as difficult to treat exhaustively as that which has been sketched with respect to the modification of empirical constructs; we shall therefore not enter

into a detailed discussion of it. The deductive aspect of the schema, however, remains always the same; what it amounts to, in the last analysis, is always that if Socrates is not mortal, either he is not human or the general premise is false. The difference is, of course, that in a theory the steps of the argument are not as simple as this and concern whole groups of empirical laws. What one does if the structure has to be mended is to redefine variables, take additional ones into account (the color of the yardstick might matter after all!), change one's assumptions as to indirectly tested empirical laws, and, if one has used any models, modify or discard them. Another noteworthy feature, finally, is that the successive theoretical organizations of a particular field which are proposed as time passes might very radically differ. For instance, an empirical law that has once been taken as basic might turn out to be not only a rather imperfect approximation, but also a remote and theoretically not very interesting consequence of the set of axioms which now proves more "convenient." This kind of discontinuity becomes particularly apparent when, as happened in our generation, atomic models are either radically modified or entirely discarded. Here, by the way, is another point where one can, at least in a descriptive manner, distinguish between empirical laws and theories. It will be remembered from **Sec. III** in what sense empirical laws can be said to be successive approximations. No such convergence, no matter how crudely and informally conceived, needs to connect successive theories, though, as a matter of fact, a general theoretical frame of reference might survive for a long time, as the mechanistic-deterministic frame of classical atomistics actually did. This discontinuity of theory is compatible, not only with the fact that empirical laws occurring at different places within the successive schemata are frequently successive approximations, but also with the circumstance that a whole theory can sometimes be said to be a limiting case of another. The existence of such a limiting process does not affect the radical theoretical or, if you please, structural discontinuity I have in mind. This discontinuity of theory construction is, to my mind, one of the strongest arguments in favor of an epistemological approach that insists on what is vaguely referred to as the fictitious or imaginary character of the theoretical particles, such as atoms and electrons.

Let us turn, now, to the electromagnetic theory of light in the form it had when originally put forth. It has already been implied that even this outstanding "theoretical" achievement lies entirely within the realm of the empirical constructs. If one should raise the objection that actually it was tied up with the idea of the theoretical medium, ether, and that it might not even have been proposed otherwise, I should use the opportunity to reassert the distinction between logical and socio-psychological (historical) analysis. In methodology we are primarily interested, not in the process of discovery, but in the fact that the theory in question can be accounted for in a certain manner. Such methodological analysis will then reveal that the electromagnetic theory of light shows considerable similarity with the case of the "three" electric currents (**Sec. III**) which were identified on the basis of empirical laws. But there are also differences between the two situations; to understand those we make use again of the notion of operational basis. In the case of the various definitions of the current the three experimental lay-outs contain common elements; indeed, except for the measuring instruments, they are the same. When the theoretical identification of optical and electromagnetic phenomena was proposed, their respective operational bases were still sufficiently different from each other to justify their being spoken about as belonging to two different areas. The operational basis for the one was, roughly speaking, light sources, screens, mirrors, prisms, and so forth, while the basis for the other consisted, among other things, of batteries, magnets, induction coils and condensers. The decisive feature they had in common was merely the mathematical structure of a very abstract empirical law which regulated the propagation of excitations and was rather fundamental in both fields. And, lest there be any misunderstanding, only the form of the two equations was the same; the operational definitions, within the two fields, of the two constructs which gave their meaning (meaning$_1$) to corresponding variables were, and for that matter still are, radically different. And there were also, within each system, equations that hitherto had not been interpreted as empirical laws by sub-

stituting the operational definitions of the other area—as for instance, the laws of reflection. Accordingly, a good deal of the testing and development of the theory consisted of the translating and testing of such "parallel" constructs and laws. Such was obviously the purpose of Hertz' classical experiments. All this is, of course, merely a methodological schema which claims neither historical nor scientific accuracy. In particular, any worth-while identification theory is likely to lead to an expansion and, finally, to a certain overlapping of the two operational bases. Development in this direction could almost be considered as the criterion for the ultimate success of this type of theory construction. Be that as it may, the point I am trying to make is merely that such theories can be accounted for within the realm of the empirical constructs. In a certain sense the electromagnetic theory of light is but a much more sophisticated instance of the technic that one applies to the three definitions of the current. Taking up a terminological suggestion that has been made before, one might say that all the constructs in question—currents, electric and optical fields, and so forth—have real referents, but that all of them refer to non-things, not to material objects. Theoretical or existential constructs, on the other hand, are fictitious or, as I should prefer to say, calculational things. What that means will be explained presently. But before we turn to the type of theory in which these theoretical or existential constructs occur, a few remarks on axiomatics are in order.

The mathematician who studies the deductive connections between formulas is not concerned with the operations which yield the substitution values for his variables, nor is he interested in the actual truth or falsehood of the empirical laws which his formulas express when the symbols are thus taken to represent empirical constructs of one kind or another. For the mathematician the wave equation, whether optically or electromagnetically interpreted, *is* the same; whatever identification takes place does not occur in his bailiwick. Generally speaking, one can develop the deductive consequences of any set of so-called axioms without ever asking whether there are any empirical constructs and laws which can be considered as their *interpretation*. Such a linguistic structure we call a deductive or axiomatic system, a scientific calculus, or, briefly, a calculus. Any pure or axiomatic geometry is a scientific calculus; analytic mechanics is another well-developed example which, by the way, if it is to be complete, must include an axiomatic geometry. Such calculi are sometimes called *formal*, but this must not be understood to mean that their axioms and theorems constitute logical or mathematical truths. Formal in this latter sense is only the deductive connection between axioms and theorems, and this, as we know, holds true of any deductive connection between statements. Therefore, if the term 'formal' is applied to scientific calculi, it merely signifies that in order to give them any empirical meaning at all, rules must be laid down which coordinate at least some of their terms to terms either of the empirical hierarchy or of the verification basis itself. Following a proposal by Reichenbach, one often speaks of these rules as *coordinating definitions*.

Like any elaborate linguistic structure, scientific calculi do contain defined terms; 'circle,' for instance, is a defined term in the usual axiomatizations of geometry. But each calculus must also contain a set of specific, undefined or basic terms, such as 'point' or 'straight line' in the axioms of projective geometry, or 'point mass' in analytic mechanics. If the basic terms of a calculus are coordinated to the terms of the empirical hierarchy, interpretations are automatically secured for the defined terms of the calculus, too. In order to find them one has merely to retrace the definitional structure of the calculus within the empirical hierarchy. It would therefore not be possible to choose coordinations arbitrarily for more than a limited set among all the terms of a scientific calculus, even if no attention were paid to empirical laws. What actually decides the acceptance of a calculus is again a feature connected with the laws: The formulas which, by virtue of the axioms, obtain between the terms of the calculus must be structurally identical with the mathematical expressions of the empirical laws which, as a matter of fact, connect the empirical constructs to which they are coordinated.[24] Loosely speaking, the two

[24] What decides the applicability of a calculus is thus a syntactical feature, namely, an isomorphism between the formulas of the uninterpreted calculus on the one hand and the meaningful formulas of our empirical language on the

sets of formulas must be the same in the sense in which the wave equation is always the same.

There is little or no point in all this as long as coordinations are given for the basic terms of the scientific calculus, since in this case the latter is merely the replica of a system of empirical laws, obtained by simply disregarding the operational definitions of the variables. Forcing oneself to think thus "formally" might be a good way to become aware of deductive connections among empirical laws, but the whole thing is really without any particular methodological significance. A new methodological problem does present itself when coordination is provided not for the basis, but only for certain more or less highly defined terms of the calculus. This is, of course, the schema of theories of the *atomic*, or *model*, type. Visualizing the terms of a calculus as a hierarchy analogous to, but different from, the hierarchy of the empirical constructs, one could say that in this case only terms above a certain level are coordinated to empirical constructs. The classical kinetic theory is the prototype of all such partially coordinated calculi. The point is that in order to achieve deductive unification between the fields of mechanics and thermodynamics it was necessary to resort to the partial coordination technic. The difficulties which this particular attempt encountered when it came to grips with the ergodic hypothesis are here none of our concern, and the tremendous deductive power of atomic theories and the role they play in modern physics are too well known to require any further comment. It is likewise obvious that I am using 'atom' and 'atomic' in a generic sense, meant to comprise molecules, atoms, electrons, the various kinds of nuclear particles and even the macroscopic continuous ether; methodologically they are all in the same category and so we are impartially interested in all of them, whether or not they happen to be now current. Characteristic of these calculi is, first, that they are but partially coordinated and that, second, their basic uncoordinated terms have certain features in common. The second point will be taken up presently. But it can already be seen that this situation faces us with the task of clarifying what a traditional philosopher might be tempted to call the ontological status of those uncoordinated calculational entities. It was mainly for the purpose of such analysis or, if you please, terminological clarification, that the distinction between a calculus and a set of empirical laws has been introduced into the present outline.

What are those features which all theoretical constructs were said to have in common? Consider, in analytic mechanics, the basic term 'point mass.' The most marked traits with which it is axiomatically endowed are, it seems to me, localization, persistence, and continuous motion along orbits. The very general character of these traits indicates that what their enumeration amounts to is really a *connotational analysis of the thing concept* and, like all such analyses, it is necessarily vague and hardly ever exhaustive. It is well to realize, for instance, that the connection between the mathematical concept of continuity on the one hand and the intuitive idea of the continuous motion of physical objects on the other is by no means simple and, moreover, is in a certain sense arbitrary. Again, it is a far cry from the relative stability of physical objects to the conservation of mass in Newtonian mechanics. Yet it seems safe to say that whenever the basic terms of a calculus do not possess some formal equivalents of these three connotations of thinghood, we do not consider the interpreted calculus as a model and are, accordingly, not faced with the "philosophical" question in what sense its elements can be said to be real or to exist.[25] This question, as we can now formulate it, concerns the status of the theoretical or existential constructs, that is, of those *uncoordinated basic terms of a partially interpreted calculus which exhibit the characteristics of formal thinghood.* In-

other. But the meaning or interpretation of the empirical constructs does not necessarily enter the picture. *Coordinating definitions* must therefore not be confused with the semantic concept of *designation*, which formalizes, in a different manner, the relation between a term and its extralinguistic referents. For elaboration of this point see G. Bergmann, "Pure semantics, sentences, and propositions," to appear in Mind **53** (1944).

[25] One could speculate that whatever the history of mechanics might have been, a stuff theory of mechanical energy could never have been proposed. For, though a conservation principle holds, mechanical energy is not localized and does not move along orbits, but resides, at least in its potential form, in the configuration as a whole. What models have actually been proposed is a subjective or historical matter; objective, however, are those features of the empirical laws which account for the possibility of a model. It is well known, for instance, how much Carnot could achieve with a stuff theory whose empirical basis was essentially the conservation principle expressed in the thermometric equation.

terpreted calculi of this kind we call atomic, or model, theories. It should be obvious by now why and in what sense this technic of theory construction has been said to be the only one that transcends the realm of empirical laws and constructs.[26]

The two notions of formal thinghood and of a partially coordinated calculus are the tools necessary to clarify the status of the atoms, a question that has not come to rest ever since Mach, Ostwald and certain French analysts reopened the age-old issue. As to the question itself the scientific empiricist will be inclined to consider it largely as a terminological matter, but let it be clearly understood that the analysis of model theories which has just been sketched is itself no verbal affair. A question of terminology is merely, how in the light of this analysis we are going to use, within the philosophy of science, such words as reality and existence. The reason it is so difficult for any terminological decision on this point to find general acceptance is that the unanalyzed notion of reality contains, among other things, at least the two ideas of being a member of the empirical hierarchy and of having formal thinghood. These two connotations are not coextensive. Many, indeed most of the empirical constructs, including some properties and all the relations of the verification basis itself, do not possess formal thinghood. On the other hand, formal thinghood is a purely structural notion and can therefore be attributed to calculational entities which need not have any connection whatever with the empirical hierarchy. The suggestion has already been made much earlier in this paper that 'real' be used, at least in the philosophy of science, as equivalent to 'belonging to the empirical hierarchy.' Then atoms are not real by definition. The reason for this suggestion is that all the essential epistemological questions which are tied in with the realism issue revolve around the status of the empirical hierarchy (always, of course, including the verification basis itself). The opinion that the status of the atoms is more closely connected with the realism issue than is any other problem of the philosophy of science is simply a sign of philosophical dilettantism.

There is one more point to be considered. One might wonder whether an existential construct of today might not be coordinated to an empirical construct tomorrow so that the whole issue is still less important than it has been made out to be. This is no doubt a very sound argument at a moment when even the layman knows that in the Wilson cloud chamber and in the Geiger counter we have almost put our finger on the individual particle. Individualization of this kind is no doubt one of the vaguer connotations of thinghood. It is certainly conceivable that one can define empirical constructs which possess many criteria of formal thinghood and can be coordinated to the basic particles of a theory. All I would insist on is that this very statement which I have just written down is a clearer account of the situation than any possible assertion about the existence or reality of the particles. And I should further insist on the obvious fact that even such a construct with formal thing-features would be derived *from*, but could never occur *on*, the verification basis of the common sense things.[27] However, the situation is even more dialectical than that. While on the one hand we have been able to put our finger on the particle, this very particle has, within the model, lost more and more of the connotations of formal thinghood. This is after all but another way of referring to what is usually called the breakdown of the mechanistic-deterministic model, so that today it even appears doubtful whether we are still making use of a model at all, and many physicists prefer what they call, rather loosely, the positivistic interpretation of their theories. What this section offers is merely the positivistic analysis, in a little more technical sense, of the terms in which physicists speak about their theories.

With this I shall rest the case which I have made, in the form of a methodological and

[26] The set of values of empirical constructs which characterizes the *state* of a physical system is, of course, not an existential construct. The concept of a state and its indices is closely related to the idea of a closed system which, in turn, is clarified under the heading of empirical laws.

[27] The fact that empirical correlates of particles would have to be *defined* constructs while the particles themselves are *basic* terms of their calculus accounts for the choice of the alternative expression 'existential construct.' For in a certain sense one cannot strictly speak about the *existence* of basic terms or particulars. In the methodological discussions at the beginning of this century these entities were referred to as *nonphenomenological*.

terminological analysis, for the thesis that there are no atoms. But it is only fair to point out that if this analysis is strictly adhered to, even stars and microscopic objects are not physical things in a literal sense, but merely by courtesy of language and pictorial imagination. This might seem awkward. But when I look through a microscope or a telescope, all I see is a patch of color which creeps through the field like a shadow over a wall. And a shadow, though real, is certainly not a physical thing. Whether such literal-mindedness is philosophical clarity or merely the extreme of positivistic indoctrination is not for me to decide.

Arthur Willis Goodspeed, 1860–1943: A Pioneer in Radiology

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IN recording the death of ARTHUR WILLIS GOODSPEED, on June 6, 1943, it seems fitting to survey briefly his scientific work, especially in its earlier and more productive period. Most of those who knew him at the outset of his career have now passed away; I welcome this opportunity to review some of his principal achievements in those early days, as a tribute to one who was for many years a congenial colleague and a faithful friend.

In 1884, when, following his graduation from Harvard, he was appointed Assistant in Physics at the University of Pennsylvania, the department was a small one, consisting of a single professor and his assistant, and occupying a few rooms in College Hall. His duties, though strenuous, gave him ample time to pursue advanced study in the newly established Graduate School of the University and to receive from it the doctor's degree in 1889, thus becoming enrolled as the first of its long list of graduates.

ARTHUR WILLIS GOODSPEED

At the time of GOODSPEED's appointment the Chair of Physics was held by GEORGE FREDERICK BARKER, a scientist of wide and versatile experience, of international reputation, and noted as a most brilliant lecturer. In those days public scientific lectures were in great demand—it will be recalled how a decade earlier JOHN TYNDALL had drawn great audiences in several eastern cities—and BARKER's lectures were noted throughout the country for their clarity and for the wealth of attractive and striking experiments with which they were illustrated. Even in his classroom he spared no pains to emphasize the salient points of his discourse by many demonstrations cleverly designed and skilfully executed.

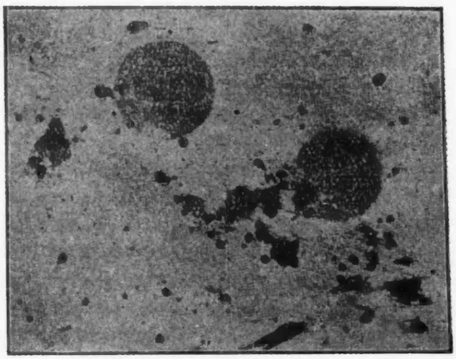
There can be little doubt that GOODSPEED's apprenticeship and association with BARKER proved an invaluable experience and that from it he drew an inspiration to carry on in his own lectures the methods of his predecessor. Throughout his career he would take much trouble—and pleasure—in illustrating new developments in physics by interesting and ingenious demonstrations.

The apprenticeship, however, was not without its trials. BARKER was an exacting taskmaster. Indeed, it used to be said around the campus that each year would bring a new lecture assistant in physics. That GOODSPEED remained with him throughout his term of office, won promotion and

ultimately became his successor, is a tribute to his tact and his equable disposition no less than to his abilities.

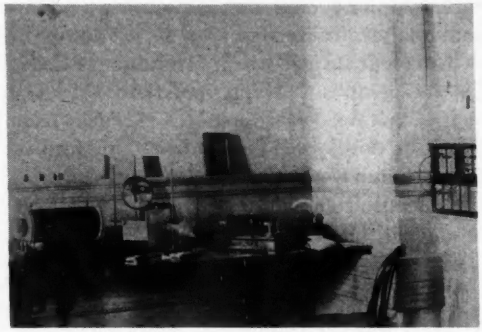
The year in which GOODSPEED came to Philadelphia witnessed the famous investigations of EADWEARD MUYBRIDGE on animal locomotion. This work, which paved the way for the modern motion pictures, was sponsored by the University and carried out on its campus. GOODSPEED's first introduction to research was in assisting at these experiments, and there can be little doubt that his great interest in the scientific aspects of photography, retained throughout his life, was stimulated, if not originated, by this experience.

A few years later we find him studying, with W. N. JENNINGS, the photography of the electric discharge. During this investigation an incident occurred that is well worth relating.[1] One evening they were recording the brush discharge from electrified coins placed on photographic plates. At the conclusion of the experiments GOODSPEED entertained JENNINGS by exhibiting some Crookes tubes, of which the laboratory possessed a fine collection. When the plates—which had been



The pre-Röntgen radiograph, taken February 22, 1890. [Science 3, 395 (1896)].

lying on the table nearby—were afterward developed, some were found to be fogged, and on one, which had not been used, there appeared some strange shadow-pictures of coins. Later GOODSPEED used to say jokingly that if he had followed up this observation we would be speak-



A corner of Goodspeed's laboratory in February 1896.

ing of Goodspeed rays instead of Röntgen rays.

When the reports of RÖNTGEN's discovery reached America early in 1896, practically every physical laboratory that happened to possess a Crookes tube set to work repeating and extending his experiments. Naturally GOODSPEED, because of his interest and experience in photography, was one of the first to enter the field. He early realized the potentially practical value of the new agent. I recall how, as early as February, 1896, at GOODSPEED's request I prepared a number of blocks of metal in some of which were hidden cavities and foreign inclusions. The description of these defects was placed in a sealed envelope, not to be opened until the picture taken by the x-rays was exhibited. Needless to say, its complete agreement with the written description formed a most impressive demonstration. The modern practice in the testing of metals for defects was thus clearly foreshadowed.[2]

But his chief interest was directed to the medical applications of the rays. For some years his attention was focused chiefly upon the development and improvement of photographic—or as he termed them, *radiographic*—records of the bony structure of the body, in particular malformations, diseased conditions and fractures, the accurate record of which is so valuable to the surgeon. In this work he was associated with many prominent physicians of the city, among them H. W. CATTELL, T. G. MORTON, J. WM. WHITE and W. W. KEEN. Indeed his interest in this field became so great that at one time he

[1] See Goodspeed's account in Proc. Am. Phil. Soc. **35**, 17 (1896); also O. Glasser, Science **98**, 219 (1943).

[2] Goodspeed, reference 1. Probably the first public suggestion of this important application.

seriously considered the advisability of himself taking a medical course.

The great success which he obtained—some of his radiographs showing remarkable detail for that early time—was due to the excellent technic which he developed. He made careful studies of the optimum working conditions of the induction coil. He also collaborated with JOHN CARBUTT in producing the first photographic plate especially designed for x-ray work, by which the time of exposure was materially reduced.

In 1900, immediately after BARKER's retirement, a quite different problem confronted him. The department was transferred to much larger quarters. A building formerly used as a girls' boarding school was purchased by the University, and the task of altering and equipping it fell largely to GOODSPEED. His ingenuity and thrift served him well in adapting, with the strictly limited funds at his disposal, a building designed for another purpose to the requirements of a physical laboratory.

With the transfer of the department to its new quarters, and the rapid growth of its activities, these reminiscences of GOODSPEED's early career may appropriately come to a close. Though he continued his work in radiography and photography, he became more and more involved in administrative duties. He served as director of the laboratory until 1931 when he reached the age of retirement. Throughout his service he upheld the standards of sound teaching and clear thinking, and was a worthy inheritor of the chair formerly filled by a succession of distinguished scholars from EWING, its first occupant, down to BACHE, FRAZER and BARKER. Those who were associated with him will always remember the courtesy and the quiet dignity with which he presided over the department for so many years.[3]

[3] For a formal obituary of Goodspeed, see H. C. Richards, *Science* **98**, 125 (1943).

American Standard Letter Symbols for Heat and Thermodynamics

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"*Aaaaaaa cccc d eeeee g h iiiiii llll mm nnnnnnnnn oooo pp q rr s ttttt uuuuu.*" This is the way in which Huygens announced a discovery in 1655. The general idea in this era was to surround a scientific statement with as much mystery as possible in order to increase the mental effort required to master it. The reader must admit the success of the plan. Such anagrams then were used quite extensively, and Hooke's law of elasticity, on the proportionality of stress and strain, was announced in 1676 in the same way. Huygens afterward restored the letters of his anagram to their proper places and published, "*Annulo cingitur, tenui plano, nusquam cohaerente, ad eclipticam inclinato.*" Being in Latin, this statement was still clouded in mystery for the average reader. The translation is, "The planet is surrounded by a slender flat ring, everywhere distinct from its surface, and inclined to the ecliptic"—the first mention of the rings surrounding the planet Saturn.

Even in recent times, it was considered desirable to becloud a statement of a scientific or mathematical fact so that a certain amount of mental effort had to be spent before a reader obtained possession of the fact, and before he could concentrate on its comprehension. But nowadays standardization of all sorts of scientific and practical things minimizes the mental effort that must be spent on the language and form of scientific expressions. An example of this is the recent proposal to standardize letter symbols used by physicists.[1]

The most important way in which this standardization helps scientists and engineers is the arrangement that all authors writing on a given subject shall use the same meanings for the

* Vice Chairman, ASA Sectional Committee *Z*10; Chairman, ASA *Z*10 Subcommittee 5.

[1] By a joint ASA and AAPT committee of which H. K. Hughes is chairman. For a preliminary report and comprehensive list of tentative symbols, see Am. J. Phys. **8**, 300 (1940).

letter symbols in their formulas. For example, *D* in a formula always denotes *diameter*. Readers readily learn such a set of standard symbols when the same ones are used in all publications, and are thereafter saved an appreciable amount of mental effort.

At the present time a university student who may be taking courses in thermodynamics, physical chemistry, theoretical chemistry and chemical engineering, usually finds an entirely different set of symbols used for given concepts in each of the fields mentioned. The mental readjustment that is required each time a student starts a new recitation gives a handicap which easily could be eliminated.

Electrical science is of comparatively recent origin, and some of the pioneers started international standardization of letter symbols for electrotechnics at such an early time in the history of its literature that practically all publications in all languages now use the same letter symbols. No such fortunate situation exists in other fields of science and technology, where great diversity exists.

ASA symbols standardization.—Some details are given beyond of a recently issued list of letter symbols for heat and thermodynamics.[2] This is part of a project initiated some years ago—"Standardization of Scientific and Engineering Symbols and Abbreviations"—under the sponsorship of the American Association for the Advancement of Science, the American Institute of Electrical Engineers, the American Society of Civil Engineers, the American Society of Mechanical Engineers and the Society for the Promotion of Engineering Education, to be carried out under the procedure of the American Standards Association. As a result, there were issued lists of American Standard Symbols in a number of fields.[3]

Scope of the ASA symbols project.—This is limited to standardization of letter symbols as used in mathematical equations and expressions. There is not included standardization of names and definitions of the concepts for which letter symbols are established, so the American Standard Association lists give only enough details about a concept to make certain what it is. But exact definitions and establishment of standard names are important matters for other committees.

Identical symbols in different fields.—In all of these symbols lists, particular efforts have been made to use the identical symbol for such concepts as are common to several fields. Workers in one field often have become accustomed to symbols of their own, regardless of usages in other fields with respect to the same concepts. But the different subcommittees have been very broadminded in avoiding such conflicts in the new lists already issued, and this is expected to continue with future lists.

A completely logical list of symbols would have a single symbol for every concept, no matter in what field it might be used, and would have all symbols selected on a sound theoretical basis, regardless of present usages. But in default of an International Dictator to enforce such an allegedly logical list, it simply would not be accepted. Lots of symbols lists which were set forth by committees who proposed to reform symbols usages have accomplished nothing beyond collection of dust on volumes of "Proceedings" in technical libraries. But we expect that the American Standard lists of letter symbols are going to merit actual use, and so we have tried to make a realistic compromise between existing usages and idealism.

Alternate symbols.—One such compromise has been the listing of alternate symbols for some concepts. These are of two sorts. In some cases there are alternates of equal rank, denoted by listing two different symbols opposite a concept, with a comma between. In other cases, a preferred symbol in the list for a given field is duly listed, and an unpreferred alternate for this field, but which is in use in another field, is given in parentheses or as a footnote. Alternates thus arranged have been found necessary in a few cases where a given concept has had different symbols in different fields, in each case with such widespread international usage that agreement could not reasonably be expected. An example is

[2] "Letter symbols for heat and thermodynamics including heat flow," ASA publication *Z*10.4–1943 (American Standards Assoc., 29 W. 39th St., New York), 55 cts.

[3] Some recently have been revised and re-issued: "Letter symbols for hydraulics," ASA *Z*10.2–1942, 35 cts; "Letter symbols for mechanics of solid bodies," ASA *Z*10.3–1942, 25 cts; "Illuminating engineering nomenclature and photometric standards," ASA *Z*7.1–1942, 25 cts. Also, references 1 and 2. Other revised lists of previously issued standards, and some new lists, are in preparation.

use of both A and S for surface or cross-sectional area.

Alternates also have been listed in cases where there have been well-established uses for a given concept, of two different letter symbols, one of which also was used for another concept in one or more lists. Then the alternate symbol could be used for one of the two concepts which have the same preferred letter symbol, thus avoiding the obvious absurdity of use of the same symbol for two different concepts in a given text. An example is the use in the heat and thermodynamics list of q for *rate of heat flow* and q and Q as alternate symbols for *rate of fluid flow*. All three symbols have had extensive use, and the listing arrangement permits use of q for rate of heat flow and Q for rate of fluid flow in cases where both rates occur in the same text.

International symbols standardization.—In many cases letter symbols are the initial letters of names. The fact that different languages use different names has started the use of completely different letter symbols for the same concept in nations using different languages. For this reason, international standardization of symbols for all fields of science and technology seems impossible at the minute. However, with people using the English language, the possibilities are much brighter.

At the present time, even though people in the United States, Canada, England and other parts of the British Empire speak nearly the same language, there is great diversity in the letter symbols used in scientific publications. This is in spite of the fact that books published in each nation are used freely in the other, that nations exchange students and that commercial concerns in each country have affiliates overseas. Hence, standardization by both nations in cooperation with the other, would be very advantageous. With this in mind the Royal Society of England arranged that the British Standards Institution start collaboration on lists of letter symbols for the English language, with other Empire standards associations, the Canadian Engineering Standards Association and the American Standards Association. The two latter bodies accepted the proposition.

The writer was in England in 1938 as delegate to a meeting at Torquay of the International Electrotechnical Commission, where symbols for electrical quantities were pretty well settled internationally. While in England he participated in the aforementioned arrangements for symbols for the English language for other fields, and definite progress was made in this direction. However, the impact of war on England presently compelled postponement of the project there. The American and Canadian Standards Associations have continued, with such information about the English point of view that it is hoped that the ASA lists being issued will require but little addition when letter symbols for the English language again come to be considered. Furthermore, the American lists will give standards for our use until this happy time comes, and will furnish a definite statement of the American point of view when it does come.

Rules for letter symbols.—As a part of the ASA symbols project there has been prepared a general set of rules for effective use of standardized letter symbols, and these "General Principles of Letter Symbols Standardization" are prefixed to each one of the independent standards for the various fields.[4] There is included a definition of a *letter symbol;* namely, that it is "a single character, with subscript or superscript if required, used to designate a physical magnitude in mathematical equations and expressions." Distinction is made between letter symbols, as thus defined, and *abbreviations*, *mathematical signs and operators*, *graphical symbols* and *chemical symbols*.

There already has been cited occasional necessity for use of alternate symbols when a given text uses concepts from different fields that have the same letter symbol. The general principles also provide for the use of subscripts to meet the same situation. Beside these principles which apply to all of the various lists for different fields, some of the individual lists have additional sets of special rules applying only to their own fields.

Acceptance of standard symbols.—Standardization in general is a very modern development. At one time every manufacturer of machinery made his parts to suit himself and used his own system of bolt threads, taper fits, and so forth. Such an arrangement was considered desirable because then new bolts and other parts for repairs would

[4] These rules appear in the lists mentioned in references 1, 2 and 3.

have to be purchased from the original manufacturer. On this basis, every author might conceivably use his own set of symbols in an effort to compel readers to use his own publications exclusively. There is no need to go into the advantages arising from the abandonment of such ideas, and the standardization of pipe threads, carburetor flanges and thousands of other machine items, as well as letter symbols.

There have been conservative people who questioned the wisdom of machine standardization. The same thing occurs to some extent with respect to the standardization of letter symbols. Some authors will ignore the advantage to their readers of the use of the same symbols which everyone else employs. Others will wait to see if everyone falls in line before abandoning their own symbol customs and adopting the standard ones. In some cases mental inertia is responsible. Often an author writes in terms of a heterogeneous lot of symbols to which he has become accustomed, beginning with his own experience as a college freshman. The more effort, however, that an author spends in making his symbols as well as his text easily managed, the more popular he will be with his readers.

A common disability with respect to symbols, unfortunately most prevalent among the better authors, is the definition, in the midst of the text, of each symbol at the first place it is used. That is, the deduction of an equation is preceded by several symbol definitions, "Let V be the peripheral velocity. . . ." Thus a leading book on the theory of gases gives, in the midst of a paragraph, ν as the symbol for molecular density. Thereafter in the formulas, ν is used without further definition. Three or four chapters on, if the reader has forgotten how ν was defined, he must go back and search through the text to find the place where it is given. This particular book commits another serious offence, by also using λ, μ and ν as direction cosines of a vector, so that the same symbol ν is used with a second, wholly different meaning. After a few equations involving ν with the second meaning, comes an equation using it with the original meaning. The author is so engrossed in his subject, and is so familiar with his own symbols, that he assumes that his readers are equally expert, which is far from being the case. The mathematics in this particular book is difficult, and possibly the author feels that any one who can master them does not need to have a symbol system easy to understand. Yet I myself, and no doubt many others, often lose hours in trying to puzzle out a mathematical text because the author has not given a definite statement covering letter symbols. Fortunately it is getting to be a common custom to include in a book or article a complete list of the symbols used. Upon completion of a few more of the ASA lists which are now under way, it is to be hoped that the editors of all technical society transactions and proceedings will specify that authors of papers must use standard ASA symbols. It is expected that publishers of technical books will be able to make a similar requirement. But even when all of this occurs it no doubt still will be desirable that each author of a paper or book include a glossary of the letter symbols which he uses.

In some texts written for a particular field, an author has chosen symbols most convenient for him at the minute, often without consideration of symbols used in other fields for his concepts. Some such books have become so popular that successive editions have been printed, and other writers have followed the symbols customs of the original author. This causes a clash when an attempt is made to standardize a single symbol for a given concept, regardless of the field in which it is used. Difficulty from such divergent practices is one of the reasons for standardizing symbols. We cannot make an omelet without breaking eggs. So when such divergent practices exist, either there must be standardized a number of confusing alternates, or some authors must give up their past practices for the general good. A number of cases of this kind have arisen in the arrangement of the symbols for heat and thermodynamics. In some cases, authors have been broadminded enough to agree to what has been shown to be a fairly general practice, despite their own previous divergences. In other cases, two different eminent authors have started out with divergent symbols, and followers of both have been reluctant to give up the divergent practices.

But we have tried to arrange a set of standard symbols for heat and thermodynamics, whether for chemists, physicists, mechanical engineers,

chemical engineers or what. In a number of cases in the present list extensive search has been made of current literature in various fields, and a symbol finally selected on the basis of majority use, or probability of acceptance by a majority. The author, as chairman of the subcommittee, has collected exhaustive lists of usages for a number of particular concepts, and will be glad to furnish copies to those interested. These lists and correspondence about them were circulated among members of the subcommittee, and a final selection was agreed upon either unanimously or by a very large majority, on the basis of best serving the general good.

The following are the selections for the thermodynamics list in particular cases where divergences have existed. These selections were based on lists of usages by various American, British and continental authorities, copies of which are available.

Energy in general, or total or molal work, has the symbol E in a good many texts. In these and in many other cases U is the symbol for internal, or intrinsic, energy. There also has been some use of E for internal and intrinsic energy. It was decided that a definite distinction between energy in general and internal energy was desirable, and so E was selected for the former and U for the latter.

A matter that caused much discussion was the selection of symbols for what G. N. Lewis and his followers have called *free energy*, $H-TS$, where H is the enthalpy, T the absolute temperature and S the entropy; and for the different thing called the *Gibbs function*, or *thermodynamic potential*, which Helmholtz called "free energy," and which is $U-TS$. The discussion arose because Lewis used F for what he called "free energy," while many continental and British writers used the same symbol F for the different concept which Helmholtz called "free energy." This causes confusion in parallel use of British or continental texts, and some American texts. F. D. Rossini, of the National Bureau of Standards, pointed out that since F had appreciable usage, with two quite different meanings, it had best be abandoned altogether. So selection was made of the symbol A, for $U-TS$, following Lewis, and of G for $H-TS$, following a great deal of usage.

Time and ordinary temperature both have had such extensive use of the symbol t that it seemed necessary to perpetuate this double usage. When both concepts appear in a single text, there has been some use of t for temperature and θ for time. On the other hand, some of the early masters of thermodynamics used θ for temperature. An appreciable majority of our subcommittee thought that from the points of view of all sorts of people using thermodynamics, it was best to continue to associate θ with temperature, and that its use as a symbol for time ought not to be perpetuated. The Greek symbol τ has had an appreciable use as an alternate for t for time, so the arrangement, approved by a substantial majority of our subcommittee and accepted by a substantial majority of other scientists and engineers with whom the matter was discussed, is as follows: t or τ for time, and t or θ for temperature, centigrade or Fahrenheit, with θ also permitted for temperature difference. This gives ample provision for avoidance of conflict when time and temperature occur in the same text.

Various ones of the symbols for radiation and humidity were subjects of appreciable discussion. A great many different sorts of people use symbols in both of these subjects, and there have been wide divergences. Complete surveys were made of usages among all the different sorts of people using a given concept, and the final selection was made on the basis of the greatest good. In the case of symbols for radiation a great deal of discussion resulted in complete agreement with committees of the Illuminating Engineering Society on a joint standard, ASA Z7.1–1942. In the case of symbols for humidity, considerable discussion resulted in pretty good agreement with meteorologists, thermodynamists and specialists in air conditioning.

The meanings attached to the words *mass* and *weight*, and the symbols for them, long have been subjects of discussion. In physics *mass* is defined by means of Newton's third law of motion or, in special cases, as equal to gravitational force divided by gravitational acceleration; and *weight* means gravitational force exerted on a body by the earth. However, Maxwell in his classical *Theory of Heat* wrote, "In all other cases, the word 'weight' must be understood to mean a

quantity of the thing as determined by the process of weighing against standard weights." Nowadays, a similar understanding is popular, and when a chemist or engineering thermodynamist or groceryman or lady on a reducing diet uses the word "weight," it has to them the significance of "quantity of matter," despite the other long established significance of "gravitational force." So the same significance exists when precise thinking engineers find the weight of a body by balancing it against standard weights on an equal arm balance or groceryman's scales. They cannot see any direct connection with gravitational force, since this "weight" is the same at any latitude or elevation, although the gravitational force varies a great deal. So our symbols list must face the fact that two different significances really are in use, regardless of arrogation by each side that they are the ones that are "right." We cannot settle which is right but we can insist on precise thinking by both sides. So the heat and thermodynamics symbols list gives specifications without any looseness for use of the words *mass* and *weight* and the symbols for them, as referring to quantity of matter; and other specifications for use of the word *weight* as referring to gravitational force. The growing practice is commended, of use when force is referred to, of units with unmistakable meaning such as the pound-force, the kilogram-force and the dyne.

A Weather Observation Exercise for the General Physics Laboratory

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A WEATHER observation exercise serves an excellent function in developing the habit of observing the skies. The experiment described in this paper was devised for use by the Preflight Aircrew students at the University of Pittsburgh, but it should serve equally well for students of the general course. However, their daily observations would not be made in the laboratory, but at some convenient place readily accessible throughout the week.

The object of the experiment is to learn to observe the weather and to practice forecasting. The laboratory directions given the students are essentially as follows.

THEORY

The weather elements are (1) barometric pressure, (2) temperature, (3) wind, (4) humidity, (5) precipitation.

Barometric pressure and winds.—Barometric lows are regions of low pressure around which the winds spiral inward in a counterclockwise direction (in the northern hemisphere). Thus the low is usually to the left of the direction toward which the wind is blowing. To locate the probable position of a low, stand with your right hand extended horizontally, with the forefinger pointing in the direction toward which the wind blows; then your thumb will point toward the low (Fig. 1).

Fig. 1. Locating the barometric low.

The position of a low on a map is determined by drawing lines through regions of equal barometric pressure. Lows usually travel from west to east at about the speed of ordinary automobile travel (30 mi/hr). As a low approaches, the barometer falls, the temperature usually rises, the dampness (relative humidity) increases and rain is predicted.

Humidity.—The *relative humidity*, or dampness, is the ratio of the density of the water vapor actually in the air to the density of saturated water vapor at the existing temperature. High relative humidity is favorable to rain.

The relative humidity is determined with the aid of some form of *hygrometer*. The hair hygrometer consists of a human hair, one end of which passes over a roller attached to a pointer; the hair stretches as it becomes more moist, hence the pointer indicates the relative humidity on a suitable scale. The wet-and-dry-bulb hygrometer consists of two thermometers, one of which has a damp cloth covering its bulb. The evaporation of water from this bulb cools it. Since the rate of evaporation depends on the dryness of the air, the readings of the two thermometers give an indication of the relative humidity.

Clouds.—Clouds consist of water droplets or ice crystals. The sizes and shapes of the clouds are indicators of rela-

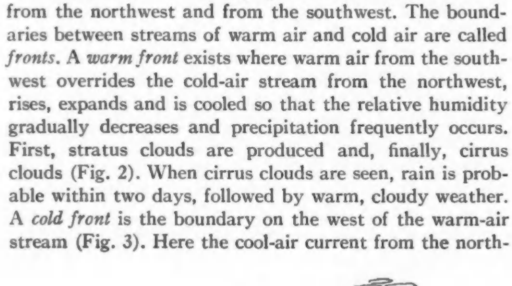
Record Form.

Date	Hour	Barometer reading (cm)	Change in 3 hr (cm)	Temp. (°C)		Relative humidity (percent)	Kind of clouds	Overcast (percent)	Strength of wind	Wind direction from	Weather prediction	Weather past 24 hr
				Max.	Min.							

tive humidity, and their motion reveals air currents in the atmosphere.

Important types of clouds (see the cloud photographs in the laboratory): *Cirrus* (Ci) clouds are thin and featherlike, and made up of ice crystals at altitudes of about 6 to 9 mi. *Cirro cumulus* (Cc) are cirrus clouds in small, round masses covering the sky, often arranged in ripples like waves at the seashore. Cirrus clouds, followed by cirro cumulus clouds, foretell rain within 24 hr, followed by cloudy, warm weather. *Formless* (F) clouds are a general overcast and include fog and haze; they usually occur in calm weather and are favorable to rain. *Cumulus* (Cu) clouds are dense, with flat bases and dome-shaped upper surfaces, at an altitude of $\frac{1}{2}$ mi or less. *Nimbo-cumulus* (Nc) are towering cumulus clouds; they are accompanied by windstorms with lightning and are dangerous to aviators. *Stratus* (S) clouds are lowlying sheet clouds.

Fronts.—An important factor in producing the weather in this country is the interplay of mammoth air currents from the northwest and from the southwest. The boundaries between streams of warm air and cold air are called *fronts*. A *warm front* exists where warm air from the southwest overrides the cold-air stream from the northwest, rises, expands and is cooled so that the relative humidity gradually decreases and precipitation frequently occurs. First, stratus clouds are produced and, finally, cirrus clouds (Fig. 2). When cirrus clouds are seen, rain is probable within two days, followed by warm, cloudy weather. A *cold front* is the boundary on the west of the warm-air stream (Fig. 3). Here the cool-air current from the northwest meets the warm-air stream but does not overrun it; being more dense, the cool air burrows under like a blunt wedge. At the cold front, violent intermixing of air occurs, often accompanied by rain with thunder and lightning. A thunderstorm is usually preceded by hot, humid weather. It is followed by cooler weather and clearing skies.



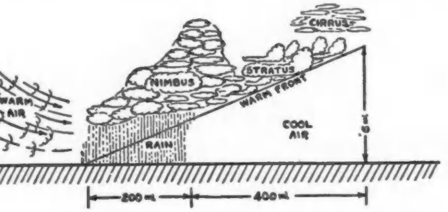


FIG. 2. (*Above*) A warm front (idealized).
FIG. 3. (*Below*) A cold front.

PROCEDURE

On each of five to ten days, the student, in addition to performing his regular experiment, will observe the weather elements, record the weather for the preceding 24 hr and predict the weather for the next 24 hr. At the end of this period, he will turn in his report to be graded in the usual manner.

(*i*) Read the recording barometer, and also record the change of its reading during the preceding 3 hr; indicate a rise by + and a fall by −. Record (*ii*) the maximum and minimum temperatures and (*iii*) the relative humidity. (*iv*) Indicate the type of clouds, if any, and the percentage of sky that is overcast by clouds. (*v*) Estimate the wind velocity and record it as: *calm* (smoke rises vertically); *light* (wind felt on face); *gentle* (leaves of trees in constant motion); *fresh* (small trees sway); *strong* (causes inconvenience to a person walking); *gale* (uproots trees). (*vi*) Record the direction from which the wind blows as indicated by a flag, smoke from a chimney or the motion of clouds. (*vii*) Predict the weather for the next 24 hr thus: *F*, fair; *R*, rain; *W*, warm; *C*, cooler; *S*, same. (*viii*) Indicate the weather of the past 24 hr. (See Record Form above.)

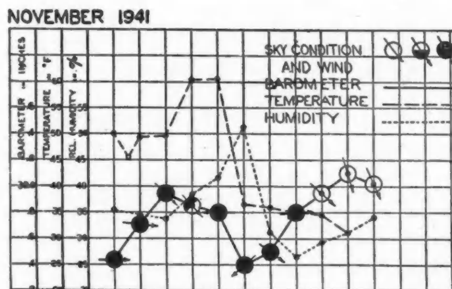


FIG. 4. Weather record, Nov. 1–11, 1941.

After all the observations have been completed, indicate on the graph sheet (Fig. 4), for each day, the barometric pressure, wind direction, percentage of sky overcast, temperature and relative humidity.

NOTES AND DISCUSSION

Efficiency of a Simple Machine

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IT is occasionally useful to know the efficiency e of a simple machine that is to be demonstrated. This can be found easily by the following trick. Taking, for example, a jackscrew with a load on it, measure the force F_+ required on the lever arm to raise the load; then measure in the same direction the force F_- required to lower it. In this particular case, F_- will be negative. Then, as can easily be shown,

$$e=(F_++F_-)/2F_+.$$

The load on the machine does not have to be known, provided only that it and the frictional force stay the same coming and going.

A Demonstration of Inertia

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THE writer finds this to be an interesting variation of a well-known demonstration of inertia. It has been used as a trick by professional magicians.

The equipment needed includes four inexpensive goblets, four eggs, four wooden cylinders of length 2 in. and diameter ½ in., and a piece of smooth stiff cardboard about 10×16 in. The wooden cylinders can be made from dowel-pin stock turned concave on one end, or sections cut from wooden thermometer cases serve nicely.

The goblets are placed at the corners of a square 8 in. on a side, and the cardboard is placed on them with one end extended towards the demonstrator. One of the wooden cylinders is then placed exactly over the center of each goblet. An egg is balanced on the top of each wooden cylinder. The object of the demonstration, of course, is to cause each egg to pass into the goblet below. At this point the magician uses a magic word, which for the physics class is "inertia." A sharp horizontal blow from the demonstrator's hand on the protruding end of the cardboard knocks it across the room, and the eggs fall into the goblets.

Ohm's Law and Joule's Law

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IN a recent article[1] Professor Perkins proposes that Joule's law, rather than Ohm's law, be chosen as a basic principle. There is, however, a method of approach to these laws that puts them on exactly the same footing.

Consider a conductor, forming a portion of a closed circuit, in which there is a constant direct current. A conductor may be defined as a material within which there are free charges, that is, charges that are free to move when a force is exerted on them by an electric field. Since the charges in the conductor are in motion, an electric field exists in the conductor and hence the potential V_a at terminal a is higher than the potential V_b at terminal b.

In time $\mathrm{d}t$ a quantity of charge $\mathrm{d}q$ enters the conductor at terminal a and an equal charge leaves at terminal b. The circulating charge therefore gives up energy of amount $\mathrm{d}q(V_a-V_b)=\mathrm{d}qV_{ab}$. This energy appears as heat $\mathrm{d}H$ in the conductor. From the principle of the conservation of energy,

$$\mathrm{d}H=\mathrm{d}qV_{ab},$$

or

$$\mathrm{d}H/\mathrm{d}t=(\mathrm{d}q/\mathrm{d}t)V_{ab};$$

and, since $\mathrm{d}q/\mathrm{d}t=i$,

$$\mathrm{d}H/\mathrm{d}t=iV_{ab}.$$

When both sides of this equation are divided by i^2, one obtains

$$\frac{\mathrm{d}H/\mathrm{d}t}{i^2}=\frac{V_{ab}}{i}.$$

This relation is a consequence only of conservation of energy and the definitions of the terms involved. *Either* side of the equation may be used to define a property of the conductor called its *resistance* R; that is,

$$\frac{\mathrm{d}H/\mathrm{d}t}{i^2}=R,\quad \mathrm{d}H/\mathrm{d}t=Ri^2;$$

or

$$V_{ab}/i=R,\quad V_{ab}=Ri.$$

There is nothing in the analysis thus far that implies that R is a constant, independent of i. In fact, there are many materials for which R is a function of i. Hence neither of the preceding equations constitutes a law; the equations merely define R.

However, many conducting materials, notably the metals, show a *linear* dependence of i on V_{ab}, or what amounts to the same thing, a linear dependence of $\mathrm{d}H/\mathrm{d}t$ on i^2. The essence of either Ohm's or Joule's law is this linear relationship, and the resistance of a linear conductor, measured for any one current, has the same value for any other current. As is pointed out by Frank,[2] Ohm's law is a statement of the behavior of many, but not all conducting bodies, and in this sense should be looked upon as describing a special property of certain materials and not a general property of all matter.

[1] H. A. Perkins, Am. J. Phys. 11, 161 (1943).
[2] N. H. Frank, *Introduction to electricity and optics* (McGraw-Hill, 1940), p. 61.

On Examinations in Physics Courses

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IN a recent paper[1] Rassweiler proposed a type of physics test for which several advantages over tests of the traditional type were claimed. The proposed test consists of four parts, in each of which the student has a wide choice among a relatively large number of questions. The

test discussed as illustrative of the type is designed for a 2-hr examination period. *Part A* consists of 30 to 40 qualitative questions on principles, laws and definitions, and has one question on every principle, law and definition introduced during the term; the student is asked to answer 10 of these questions. *Part B* consists of 25 to 30 simple problems, each involving only a single physical principle, and there is one problem for each principle studied during the term; the student is asked to solve 10 of these problems. *Part C* consists of 8 to 12 problems, each involving the combination of several physical principles. The student chooses two of these or one of the problems in *Part D*, which consists of a group of four to six "bonus problems" presumably of a difficulty greater than the average student can be expected to handle. The weighting of the different parts of the test discussed, and apparently considered reasonable, is 30 percent for Part A, 50 percent for Part B, and 20 percent for either Part C or Part D.

An inspection of the questions and problems which are cited as examples of the types used, shows that the questions of Part A can be correctly answered by a student who has memorized the definitions of a number of physical quantities and the statements of a number of physical principles, and that the problems of Part B, being of the type which can be solved by the substitution of numbers into a formula found in the textbook, may be correctly solved by a student who has memorized a number of formulas. The problems of Part D, and to a lesser extent those of Part C, are of a type that can be solved only by those students who understand the principles involved. In the traditional type of test a student who has what might be considered a satisfactory grasp of the principles of physics would be expected to be able to solve two or three problems of this type in an hour, or six to eight in a 3-hr examination.

With the weighting suggested it is possible for a student relying upon memory alone, and with little or no understanding of physical principles, to score 80 percent. This possibility seems to be recognized by the proponent of the test, but apparently the advantages claimed outweigh any considerations of this sort.

One of the advantages claimed for the test is that it "does a much better job of evaluating student effort and accomplishment;" another, that it encourages greater effort on the part of the student since he knows that anything he learns "may be placed before the instructor for credit." Whether these advantages are real or not depends upon the kind of accomplishment evaluated and the kind of effort encouraged. Certainly as teachers we are not interested in making students work because work is a good thing in itself. Nor are we primarily interested in training the student's memory. Conceivably, the memorization of "Gallia est omnis divisa in partes tres, quarum unam incolunt Belgae, aliam Aquitani, tertiam, . . ." may be at least as effective in training the memory as the memorization of the statement that "an unbalanced force acting on a body causes the body to accelerate in the direction of the force, and the acceleration is directly proportional to the unbalanced force and inversely proportional to the mass of the body." What we are interested in having the student acquire is an *understanding* of the fundamental ideas and principles involved in the explanation of physical phenomena. A proper examination should test this understanding in the only way in which it can be tested—by means of problems that the student can solve only by giving careful thought to the principles involved. The type of test proposed, with the weighting heavily favoring Parts A and B, is of doubtful value as a means for measuring ability to think in an exact way about physical phenomena.

One of the reasons prompting the development of the type of test proposed appears to have been the desire to do something about the "bad repute" physics has among students. The use of the new type of test would, it is implied, reward and encourage those who are willing to work hard, and by raising the general level of grades in physics courses improve the reputation of these courses on college campuses. The supporting argument advanced that a student who "has done all that could be expected of his best effort" is entitled to a respectable grade in the course smacks too much of the school of thought which holds that the necessary and sufficient condition for a college degree should be four years (less under an accelerated program) of residence on a college campus. It would be most unfortunate if teachers of physics, in order to win friends among the students, found it necessary to accept effort or seriousness of purpose as a substitute for understanding. Appeasement is supposed to have gone out of style after Munich.

In evaluating a student's accomplishments in the course at the end of a quarter, semester or academic year, the effort he has made and the time he has spent in studying physics are quite irrelevant except, perhaps, in borderline cases. The one pertinent question about the student is, "How well does he understand the principles of physics?" The tests given and the exercises required of the student during the term should be of a type that will enable the instructor to answer this question intelligently.

[1] M. Rassweiler, Am. J. Phys. **11**, 223 (1943).

Reply to the Preceding Note

WATSON'S criticism of my paper seems to be directed in part at the motive which led to the development of this testing method—this he terms appeasement. Raising and spreading numerical grades merely to conform to the usual grading standards might be considered appeasement of the dean's office; but when it is undertaken in an effort to evaluate more accurately the student's accomplishment, the charge of popularity seeking is hardly justified.

I think Watson has overlooked the fact that we teach physics at several different levels in different courses, and that "a satisfactory grasp of the principles of physics" and the principles to be grasped differ with the course level; as do also "the kind of accomplishment evaluated and the kind of effort encouraged." Giving such a test as Watson suggests at certain elementary levels usually results in an average grade of 30 to 40 percent—an outcome that some teachers consider necessary for maintenance of high stand-

ards. An examination on which most of the examinees fail most of the questions may indeed be useful in restricting immigration, or in reducing the number of premedical students, but it is not a criterion for academic accomplishment. Such tests destroy morale and force a lowering of standards. Possibly, however, the whole criticism is based on the unjustified inference that raising and spreading numerical test grades necessarily implies giving more *A*'s and *B*'s.

Most of the criticism is directed at the particular test from which some examples were taken. Let me reiterate that this test is only *one example* of *one way* that the method of testing has been applied. As I pointed out in the original paper, the number of parts, their weights and the particular questions used would naturally have to be worked out differently for different courses and different institutions.

No meaningful discussion of a particular test can be undertaken without considering the level of the course, the manner in which it is taught, the ultimate objectives of the students and other similar factors. The influence of these factors on the development of a test for a particular course under any testing plan is a whole new field of inquiry quite beyond the scope of this reply. Two points, however, surprise me so much that I feel some answer is necessary.

Watson objects to the Part A questions apparently because he thinks that it is never justifiable to ask for statements of definitions and laws. Does it not then follow that he would never consider acquiring a knowledge of such statements a worthwhile accomplishment? Surely he does not mean to imply that understanding can be achieved without accurate definition of terms and concise statements of basic principles. This would smack of the school of thought which, when applied in elementary and secondary schools, sends us freshmen who "understand" complex numbers but cannot solve simple equations! Learning a well-worded definition of a basic principle after much study and discussion is a totally different thing from mere memorization of "Gallia est omnis. . . ." The first clarifies and clinches an idea and lays a firm foundation for further scientific thought, while the second has nothing to do with physics. If on the other hand, Watson would agree that such exact statements are necessary to understanding, then the student must know them, his knowledge of them is part of the required accomplishment, and tests for them are justifiable.

We are also surprised at the reappearance of the myth that problems involving measured quantities can be solved merely by memorizing formulas. All of us have encountered cases in which a student came to a final examination equipped with all the formulas, but could not solve even simple problems because he did not possess certain essential associated knowledge; he did not know what formula to use where, what the symbols meant, what restrictions on units existed and what units resulted. To work even a problem on first principles a student must *learn* certain relationships either by means of a cumbersome word-thought pattern, sometimes confused with understanding, or by means of a compact algebraic expression with certain associated ideas. The use of the latter method is a matter of plain efficiency which has been much in vogue since appeasement went out.

The relative emphasis to be placed on definitions, laws or problems based only on first principles depends upon the level at which a student is attacking the subject; and this, as I said before, is another subject entirely.—MERRILL RASSWEILER

RECENT PUBLICATIONS

IMMEDIATE AND ADVANCED PHYSICS

Fundamentals of electric waves. HUGH HILDRETH SKILLING, Professor of Electrical Engineering, Stanford University. 193 p., 65 fig., 15×23 cm. *Wiley*, $2.75. This textbook for an upper division course is intended to provide an introduction to more advanced work on electromagnetic theory as well as background for understanding in radio engineering. The only prerequisites are general college physics and elementary calculus. Vector methods are used but are adequately explained in a preliminary chapter. About half of the book is on electrostatics and other basic material. Then, as a logical conclusion of this work, Maxwell's equations are presented and radiation and wave propagation are developed. Finally, antennas, transmission lines and wave guides are briefly discussed. The problems are numerous and in most cases serve to develop some idea that is purposely left incomplete in the text.

The physics of metals. FREDERICK SEITZ, Professor of Physics, Carnegie Institute of Technology. 343 p., 189 fig., 14×21 cm. *McGraw-Hill*, $4. The outgrowth of an evening lecture course given by the author during the last four years to mixed groups of metallurgists and physicists, each with a limited knowledge of the other's field, this authoritative discussion of metals has intentionally been kept both nonmathematical and elementary. Yet the whole point of view is that of atomic physics, with consequent emphasis on the ultimate nature and fundamental aspects of metals. As the author actually found to be the case with the practicing metallurgists in his classes, most practical workers will exhibit great interest and curiosity in the fundamental aspects of their field, if only someone will take the trouble to supply them with material that is understandable. Some of the main topics discussed in the book are atomic arrangements in metals, the periodic chart, the physical

form of alloys, elastic properties of crystals, plastic properties of crystals and alloys, creep and secondary plastic effects, internal friction, rupture and fatigue, diffusion and solubility of gases in metals, electron theory of metals, band theory of solids, cohesion of solids, magnetic and electric properties of metals.

Heat and thermodynamics. MARK W. ZEMANSKY, Associate Professor of Physics, College of the City of New York. Ed. 2. 390 p., numerous diagrams and tables, 15×22 cm. *McGraw-Hill*, $4. The scope and character of the first edition [Am. J. Phys. **6**, 339 (1938)] are preserved here, but there are many new problems, several topics entirely new to the book—convection, entropy and nonequilibrium states, second-order phase transitions, superconductivity thermal capacity of reacting gas mixtures, and the Le Châtelier principle—and a number of amplified topics—notably, the temperature concept, the first and second laws, the phase rule, and various experimental methods. The book contains thoughtfully selected material and is eminently suited for use as the basic textbook in an intermediate course for students of physics, theoretical chemistry and engineering who have had only general physics and elementary calculus. Some material not ordinarily covered in the prerequisite courses is occasionally employed, but is developed as needed. About half of the text deals with fundamental principles; the remainder, with applications. No statistical mechanics or kinetic theory are included, partly because of the mathematical difficulties, but also because the author believes that the macroscopic and microscopic points of view are better left unmixed at this level of training.

Treatment of experimental data. ARCHIE G. WORTHING, University of Pittsburgh, and JOSEPH GEFFNER, Weirton Steel Company. 351 p., 13 tables, numerous diagrams, 15×23 cm. *Wiley*, $4.50. Probably every physical scientist has in the course of his work experienced irritations similar to those described by PROFESSOR WORTHING in the Preface to this book; namely, of too often encountering tables of unsmoothed values, tables and graphs that lack suitable legends, graphs with poorly chosen coordinate scales, reference to the significance of a so-called knee of a curve when the location of the knee was a function of the chosen coordinate scales, lack of understanding of how to determine, express and apply precision indexes, blind faith in a least squares computation regardless of the assumptions and limitations, and many other faults which the present authors demonstrate can be remedied with reasonable effort. In their book they provide chapters on tabulations of data, graphs, representation of data by equations, tabular and graphical differentiation and integration, determinants as a means of simplifying computations, Fourier series, the normal frequency distribution, means and precision indexes of unequally weighted measurements, propagation of precision indexes, adjustment of conditioned measurements, least-squares equations representing observed data, the essentials of correlation, and analysis of nonharmonic periodic functions. A list of problems accompanies each chapter. The book is the outgrowth of the senior author's course on treatment of experimental data. Carefully planned and written, it is suitable for use either as a textbook or as a reference work for physicists, chemists and engineers.

GENERAL COLLEGE PHYSICS

Physics. ERICH HAUSMANN and EDGAR P. SLACK; Naval Academy edition, revised by EARL W. THOMSON, Professor of Physics, U. S. Naval Academy. 961 p., 438 fig., 13×21 cm. *Van Nostrand*, $5.50. Although Hausmann and Slack's well-known textbook has been used since its inception [Am. J. Phys. **6**, 107 (1935)] in the general physics course at the Naval Academy, local requirements make it desirable to amplify certain topics—projectile motion, simple harmonic motion, momentum, thermodynamics, optical instruments, and so forth—and at the same time to reduce the emphasis on certain parts of electricity that are covered in later courses on electrical engineering. These changes were formerly effected by furnishing the midshipmen with supplementary notes, which the present special edition now renders unnecessary. About 200 pages have been added to the second edition of the original book—a book, incidentally, that has gone through 22 printings.

Student's handbook of elementary physics. ROBERT BRUCE LINDSAY, Professor of Physics, Brown University. 397 p., diagrams, 14×21 cm. *Dryden*, $2.25. Numerous technical handbooks of various kinds and grades of difficulty are of course available, but this one is novel in that it has been prepared especially for the use of students in general physics courses. The handbook opens with a brief essay on how to study physics and then proceeds to the two main parts: (*i*) a nonmathematical review, "advisedly termed a Primer," of physics essentials that the elementary student ought to grasp, and (*ii*) a dictionary, illustrated with diagrams, of all the physical terms that the student is likely to encounter. Included in the Primer are a number of worked examples, numerous problems with answers only, and many very simple experiments for the student to carry out. An extensive Appendix contains a chronological outline of physics history and of contemporary events in other sciences and in general history; a bibliography of suggested reading; a collection of useful formulas; and tables of physical and mathematical constants. The author points out that the handbook is intended primarily to supplement a textbook, although it might serve of itself as a brief text for short courses.

Fundamental physics. LLOYD WILLIAM TAYLOR, Professor of Physics, Oberlin College. 715 p., 528 fig., 15×24 cm. *Houghton Mifflin*, $4. Considerable basic material and numerous other features of the author's outstanding book, *Physics: the Pioneer Science*, have been carried over to this new textbook for wartime general physics courses. Although the strictly historical material has been reduced and modified, the historical approach has been retained in spirit. Even in wartime, not to speak of preparation for the peace which is to follow, it is essential that students of college caliber gain more from their physics course than a specialist type of training; and many competent teachers hold that

the needed perspective, involving increased comprehension of the broad implications and limitations of physical science, of its social and humanistic aspects, can be successfully imparted through a skilful historical approach, yet with little, if any, reduction in the amount of traditional subject matter taught. In the present book, unlike the earlier and broader one, electrostatics follows current electricity and is reduced in extent. Numerous footnotes have been eliminated by putting the literature references in an appendix and referring to them by serial numbers placed in the text proper. Most of the problems are formulated in algebraic terms, with several sets of numerical data for each example, so that different sets can be used in successive years. Four of the chapters, on modern physics, were written by F. G. TUCKER; the one on radio communication, by C. E. HOWE.

MATHEMATICS FOR ENGINEERS

Calculus. LYMAN M. KELLS, Professor of Mathematics, U. S. Military Academy. 517 p., many diagrams, 15×23 cm. *Prentice-Hall*, $3.75. A textbook on elementary calculus. The numerous problems are graded in difficulty; many are novel and quite physical in character. More than ordinary use is made of diagrams, many of them illustrating physical situations.

Engineering problems illustrating mathematics. JOHN W. CELL, Associate Professor of Mathematics, College of Engineering, North Carolina State College. 183 p., 153 fig., 15×23 cm. *McGraw-Hill*, $1.75. This published collection of 511 problems, with answers, has been issued on a nonroyalty basis as the culmination of work begun in 1938 by a committee of the Mathematics Division, Society for the Promotion of Engineering Education. A lithoprinted edition was circulated in 1941–1942, and the resulting criticisms and additions have been incorporated in the present printed edition. The problems are planned for use in the ordinary undergraduate mathematics courses for engineering students and are grouped according to courses, from college algebra through integral calculus. They are intended for use as supplementary material, and not to supplant the usual exercises or to render the mathematics courses solely utilitarian. Their purpose is to give students some understanding of the use of mathematics in engineering and hence of the need for a thorough foundation in the field; in other words, to help them to answer the perennial questions as to why various topics should be studied. Most of the problems are suitable for direct assignment to superior students, and many can be used with those of only average ability.

WAR COURSES

The practical essentials of pre-training navigation. WILLIAM T. SKILLING, San Diego State College, and ROBERT S. RICHARDSON, Mount Wilson Observatory. 118 p., 19 fig., 14×21.5 cm. *Holt*, paper covers, 75 cts. A well-written account, mainly descriptive, of the elements of celestial navigation, meteorology and map projection.

Basic physics for pilots and flight crews. E. J. KNAPP, Professor of Mathematics and Physics, Texas College of Mines. 123 p., numerous diagrams, 13×19 cm. *Prentice-Hall*, $1.25. A condensed presentation, with problems, of the elements of mechanics and heat needed for meteorology, theory of flight and engine operation. It is intended to meet the requirements of primary flight or air cadet training.

Pre-service course in machine science. SAMUEL H. LEBOWITZ, Straubenmüller Textile High School, New York. 448 p., 219 fig., 14×21 cm. *Wiley*, $1.96. This, the latest volume in the *Wiley pre-service series* [Am. J. Phys. 11, 168 (1943)], has 14 chapters that conform in content to the corresponding sections of the War Department and U. S. Office of Education syllabus on fundamentals of machines. The treatment is along traditional lines; the writing and the book work are good.

MISCELLANEOUS BOOKS

Youth looks at science and war. 141 p., 11×18 cm. *Science Service* and *Penguin Books*, paper covers, 25 cts. Contains the 40 winning essays by youth aged 15 to 18 in the first Annual Science Talent Search of Science Clubs of America; also, the 100-question science aptitude examination, with answers, that was administered to all participants.

Heat treatment of metals. J. WINNING. 99 p., 39 fig., 14×21 cm. *Chemical Publishing Co.*, 50 cts. This concise account of modern heat treating methods and of the principles underlying them is designed primarily for the use of nonspecialists in metallurgy who must deal with heat treatment and its problems in their everyday work. The main topics discussed are general principles, furnaces and equipment, casehardening, hardening machines, hardening carbon and alloy steels, stainless and rustless steels, nonferrous alloys, and procedures.

A guide to cathode-ray patterns. MERWYN BLY, Associate Engineer (Radio), Navy Department. 46 p., 183 fig., 22×27 cm. *Wiley*, paper covers, $1.50. Oscilloscope operators should find ready use for this practical "sketch-and-caption" summary, without detailed theory and analysis, of cathode-ray pattern types that are likely to be encountered in laboratory and test bench work. The patterns are grouped as follows: frequency determination; modulation; sine-wave testing; square-wave testing; resonance; vacuum tube characteristics; miscellaneous patterns. A section on simple graphic analysis is included.

Workbook in meteorology. ATHELSTAN F. SPILHAUS, Professor of Meteorology, and JAMES E. MILLER, Instructor in Meteorology, New York University. 172 p., many diagrams and work charts, 23×28 cm. *McGraw-Hill*, paper covers, $3. The 37 exercises, or projects, in this looseleaf manual are designed to supplement textbook material in meteorology courses of college grade and technical

character. The exercises are arranged in four groups: mean condition of the atmosphere; instruments and observational methods; dynamical meteorology; weather-map and upper-air analysis. The technical quality and presentation of the material appear to be excellent.

The microscope and its use. FRANK J. MUÑOZ, Technical Microscope Consultant, and HARRY A. CHARIPPER, Professor of Biology, New York University. 346 p., 152 fig., 14×21 cm. *Chemical Publishing Co.*, $2.50. Designed to fill the gap between the pamphlets issued by manufacturers and the large textbooks, this guide to the use of the microscope is replete with specific, detailed instructions of immediate value to technicians and students. The language is nontechnical; the treatment of the optics involved is very elementary. The main topics dealt with are the history of the microscope; the structure, use and care of the modern instrument; illumination; the microtome; microscope accessories; and stereoscopic, metallurgical and polarizing microscopes. Numerous common errors in the use of the instrument are discussed. A glossary of terms and a selected bibliography complete the book. The illustrations and book work are excellent.

Visual mechanisms. Edited by HEINRICH KLÜVER, Professor of Experimental Psychology, University of Chicago. 330 p., many illustrations, 18×25 cm. *Jaques Cattell Press*, $3.25. Appearing as Volume VII of *Biological symposia*, these 12 technical essays by 15 different authors survey problems of vision from physical, biological and psychological points of view. They are meant to give a picture of the diverse methods and viewpoints found in modern work on vision, rather than to deal exhaustively with all phases of the subject. Eight of the essays are expansions of papers read at the symposium on visual mechanisms held at the University of Chicago in September, 1941. Perhaps of most interest to physicists is the essay by SELIG HECHT on "Energy relations in vision," which describes direct experiments leading to the conclusion that the minimum energy needed for vision under optimal conditions corresponds to only 5 to 14 quanta of blue-green light actually absorbed by as many retinal cones.

DIGEST OF PERIODICAL LITERATURE

Magnification and Brightness of Retinal Images

Perhaps every teacher of optics has tried to convince some doubting student that an opera glass makes an object seem larger but never brighter. One hastens to add that the student who expresses his doubts has a point in his favor; the act of placing an opera glass in front of the eyes cuts off lateral illumination, and the iris may then open and admit more light. But this is physiology, not optics, and does not alter the case. One peculiar feature of this question of brightness is the silence of the textbooks; the writer has seen but one reference and that one is wrong, since it states that a near object is brighter because the iris collects more light from a nearby surface!

With the help of a ray diagram it can easily be shown that the linear magnification of an ordinary opera glass is equal to the ratio of the focal length F of the positive lens to the focal length f of the negative lens. Hence, when the eye looks directly at the object and when the opera glass is used, the areas of the two images on the retina are in the ratio F^2/f^2. But it can also be shown that the light collected by the eye in the two cases is as F^2 to f^2. Since this is the identical rate of increase found for the two images, the density of light in the two is equal and they are equally bright.

In a microscope the diameter of the emergent pencil may be less than that of the pupil of the eye, and in this case the image seems less bright. But ordinarily, either in opera glass, magnifying glass or telescope, the equality of brightness is preserved, or as closely preserved as the surface and absorption losses will permit.—FRANK BENFORD, *J. Opt. Soc. Am.* **33**, 245 (1943). D.R.

Spreading Monomolecular Films

A microscope slide is cleaned with soap or cleaning solution so that water wets the surface uniformly. The slide is held vertically with the fingers at the bottom, and tap water is allowed to flow over it so as to remove all surface films. The slide is then removed and the water allowed to drain away. As it does so a slight ripple is observed to start from the fingers and move upward along the water surface, taking about 15 sec to travel the length of the slide. The ripple marks the advancing edge of a film arising from the fingers and spreading over the water surface; it is caused by the reduction in velocity of the water due to the viscous drag on the film. The experiment can be repeated as often as desired simply by holding the slide under the tap again.

The same result can be observed when water is allowed to flow continuously down a vertical stirring rod into a container of water on the surface of which is a film. The film forces its way up the flowing water surface until its expanding pressure is balanced by the viscous drag of the water moving beneath it. A small amount of nonwetting powder, such as aluminum stearate, on the film makes the experiment much more graphic. The stirring rod is not essential to the success of the experiment, but its use enhances the effects. The container into which the water flows should have an overflow pipe whose outlet is beneath the water surface and which is then bent upward to a point slightly below the rim of the vessel. Thus the water surface can be cleaned by stopping the overflow pipe and allowing the water to run over the edge of the vessel; or

when a film is formed it can be kept on the water when the overflow pipe is open.—B. VONNEGUT, *J. Chem. Ed.* 20, 292 (1943). J.D.E.

"Bold and Broad" Mathematics

Oliver Heaviside, self-taught and working in isolation, took no trouble to conform with recognized mathematical methods, and even seemed to take a delight in apparently unsound arguments. "Mathematics," he said, "is of two kinds, rigorous and physical. The former is narrow: the latter bold and broad. To have to stop to formulate rigorous demonstrations would put a stop to most physico-mathematical inquiries. Am I to refuse to eat because I do not fully understand the mechanism of digestion?"

This attitude aroused great opposition from the pure mathematicians. But there was something to be said on their side; Heaviside himself said, "even Cambridge mathematicians deserve justice." Ever since the introduction of the differential calculus, mathematicians had been tempted to rely on intuition rather than on rigid proof. By this means they speedily obtained a large number of results, but unfortunately many of these were not altogether accurate. The error generally consisted of the assertion as a universal truth of a theorem that actually held good only under certain conditions. Only in the later part of the nineteenth century had Weierstrass and Dedekind succeeded in abolishing the slipshod methods of the preceding 200 years, and in returning to the high ideals of mathematical rigor held by the ancient Greeks. To those who took part in this reform, Heaviside's methods seemed a kind of mathematical blasphemy, a wilful sinning against the light. Yet Heaviside's results were always correct! Could a tree be really corrupt if it always brought forth good fruit?

A similar situation arose early in the nineteenth century. Fourier expanded functions in an infinite series of harmonic terms, and applied the results to problems in thermal conduction. The results were very valuable, and yet the methods used were unsound, as was pointed out at the time by Lagrange. The investigation of the reason for the success of Fourier's work was difficult, but it ultimately led to great advances, and may be taken as the starting point of the theory of functions of a real variable. Possibly with this in mind, pure mathematicians of the present century, when the battle for rigor had been won and the victors could afford to take up a more tolerant attitude, began to show more appreciation of the "bold and broad." G. H. Hardy, some time before 1914, said that what analysis then needed was a twentieth century Euler, capable of trying daring experiments with what one might call "conjuring tricks in mathematics;" the details of justification might then be filled in by workers at their leisure.

Bromwich considered that Heaviside must be ranked with the greatest constructive mathematicians of the nineteenth century, far outstripping the finest senior wranglers of the Mathematical Tripos in his amazing skill in manipulation.—An excerpt from H. T. H. PIAGGIO, "The operational calculus," *Nature* **152**, 93 (1943). D.R.

On the Making of a Research Worker

Few today would question that the student of applied science who aspires to become a research worker should be given special graduate training. But the apprenticeship in research—that is, independent research leading to a dissertation—is not the only conceivable method for giving such special training. It is quite feasible to give direct instruction in the general principles of research, in the special mathematics of experimental work and in the modern technic of research in a particular subject. Indeed, by means of such courses one can give the student a more complete equipment of the special technical knowledge he needs than is possible under the apprenticeship system. Yet a real apprenticeship period is just as essential in the ideal training of a research worker as is the direct formal instruction in the research technic of his subject. It is necessary as a means of educating any gifts of scientific thoroughness, judgment and outlook with which the student has been endowed; later work as a junior in a research institute is not the same thing as a proper apprenticeship.

In a recent article[1] it is stressed that the clear objective of the apprentice system must be the training of the research worker, not the conducting of research; and it is rightly pointed out that the temptation must therefore be avoided to use the research student as an observer in other men's research work—the student must not be an extra pair of hands for his tutor. It is also pointed out that the god of "communications" is a false god whose worship must be avoided. But no mention is made of yet another cult that can be carried too far, namely, that of the dissertation.

The striking difference between the conditions under which a man works while a research student and those under which he will work later is that in the former capacity he has perforce to produce his dissertation at a certain fixed date, marking the end of his apprenticeship. He is thus instructed, as it were, to "publish his results" as early as possible. Yet when he becomes a paid worker he will be, or should be, equally strongly discouraged from anything savoring of premature publication. The preparation of a dissertation unquestionably is an essential part of the apprenticeship system. But unless special care is taken, it can easily become the chief object, whereas his object during that time should be to do good work, and the dissertation is but the record of it.

It is mentioned[1] that his own mistakes are part of the apprentice's training. So also should be the mistakes made by earlier students in the same institution, as recorded in the filed dissertations on problems related to his own. The extreme degree of conciseness and elimination of nonessentials that is so desirable in publications nowadays need not be required in a dissertation. This makes a body of earlier dissertations particularly instructive.

On the administrative side, a most important factor is a high proportion of "masters" to "apprentices." The number of students that one research supervisor can handle satisfactorily must vary to some extent with his subject and the kind of problems he gives to his students; but it is

doubtful whether there ever can be the best possible guidance if the number supervised by one tutor exceeds five. In some subjects it must be even smaller. Thus the training of research workers, if it is to be really good, must be expensive. But the men trained should be very valuable material that is being made into something of first-rate importance; therefore nothing should be spared that will contribute to the making. It would be hard to conceive of any more interesting and satisfactory duty for a scientific man than the making of a research worker out of a good student. In order not to end on a note of pessimism, we must say nothing about the supervisor's feelings on the rare occasions when he suffers from an ill-chosen "apprentice."—O. T. FAULKNER, *Nature* **151**, 332–333. D.R.

[1] Editorial, *Nature* **150**, 245 (1942); digested in Am. J. Phys. **11**, 52 (1943).

Adequate Physics Apparatus for Every School

Super-salesmanship and generous administrations will never be responsible for solving completely the problem of inadequate science equipment in secondary schools and small colleges. The instructors themselves are often responsible for the lack of equipment—they usually prefer to spend their budgets on many small pieces of equipment rather than to plan and execute a long-term buying program. Because high frequency apparatus, radio demonstration equipment, oscilloscopes, crystal and molecular models, optical apparatus, and so forth, are expensive and used only a relatively few days during the year, most small schools never buy them.

There is a way by which each school can be supplied with all the laboratory and demonstration equipment it needs. This is to *rent* it to the schools in much the same way that films and slides are now rented and reserved for certain weeks. One can visualize a truck traveling from school to school like a traveling library; but instead of books, it carries specially constructed boxes housing sets of apparatus and equipment. A skilled apparatus man would set up any of the equipment which required special attention, would be able to give advise and instruction on its use, would act as a consultant and recommend teaching methods and demonstration procedures for future topics and sections.

The inexhaustible supply of equipment would assure the instructor of having every piece of equipment a textbook or manual might suggest. With an adequate array of equipment available, he would become more valuable to the school because he would become a better physicist and a better teacher. The resulting, improved physics course would in turn favorably advertise the school and its science program to students and parents.

Schools subscribing to such a service would list the experiments which they would like to conduct along with suggested dates for the demonstrations. The *distributing agency* in this plan would: (*i*) stock the equipment needed for all the cooperating schools in its area, (*ii*) maintain it in excellent repair, (*iii*) make suggestions for its use and care, (*iv*) suggest alternate experiments, innovations and additional experiments, (*v*) maintain a staff of advisers who would prepare manuals to aid the teacher in his laboratory and demonstration work and who would also be able to answer technical questions.—SHAILER PETERSON, *Sch. Sci. and Math.* **43**, 451–454 (1943). AUTHOR

Number of Physicists in America

A typical example of the difficulty of estimating the number of scientists in war work may be found in an illustration from the field of physics. Experience of the National Roster indicates that the physicists are as well organized, if not better organized, than any other professional scientific group. It is also probably true that the National Roster has more complete records concerning the members of the profession of physics than in any other field. From the first, however, the physicists themselves have found it difficult to establish a fully satisfactory definition of what constitutes a physicist. A minimum definition of a *professional physicist* has been presented by the physicists as an individual with at least a master's degree or one possessing equivalent qualifications based on a combination of training and experience. There are somewhat more than 7000 such men and women in America. Despite this definition, many physics majors with bachelor's degrees have been employed in war research and as college teachers and as such have been treated by the National Roster as professional physicists. This group, if added to the 7000 previously mentioned, might justify the statement that there are probably 11,000 physicists. On the other hand, if the definition of scientist adopted indicates an ability for advanced independent research, there are probably not more than 2000 or, at the most, 3000 physicists in America.—Excerpt from L. CARMICHAEL, "The number of scientific men engaged in war work," *Science* **98**, 144 (1943). D.R.

Subjective Origins and Objective Outcome of Physical Experiments

In the morning, the scientist leaves his home, goes to his laboratory, and shuts himself in—kissing his wife as he leaves, greeting his colleagues pleasantly as he arrives, and taking off his coat and buckling down to work as he closes the door. In passing, we may note the subjective determinants of this early morning behavior: he kisses his wife to express regret for the coming day of separation, he pleasantly greets his colleagues to placate them for the apparatus he will swipe before night; and he buckles down to work to satisfy his intellectual curiosity, economic needs, ambition or some general urge to achieve. But what determines the program of work that he undertakes? At least on the days of great discovery, he selects this program —mind you—not to obtain objective results as rapidly as possible, but to satisfy his own subjective needs to test some hypothesis or to prove some theory.

The good physicist on entering his laboratory does not say, "I shall diligently use this apparatus to make measurements of the kind for which it is fitted and obtain with dispatch objective values that can be commonly accepted." Rather, he says things such as the following: "I shall find out whether an electrostatic charge set into motion does have the properties of a current." "I shall look for the

effects which should result from the earth's motion through a stationary ether." "I shall see if the electrons in a conducting metal do exhibit mechanical inertia." "I shall look for the spatial orientation of atoms in an external field so astoundingly predicted by the quantum theory." "I shall find out whether the neutrinos which have been postulated to conserve energy in radioactive transformations would also act so as to conserve momentum." The origin of such problems is a subjective one, and the great physicist is the man who has a feel for problems that are both significant and soluble.

At night, as the physicist walks home, he thinks over the doings of the day and questions the objective validity of his results. "Were those meter readings due to some leak in my circuits, or really caused by the motion of the charge on my rotating disk?" "Does that group of consistent interferometer readings actually mean a positive effect, or is it really properly explicable merely as a statistical fluctuation?" "Are those occasional wild galvanometer throws actually of significance, or really due to unimportant accidental causes?" "Were those lines on my plate merely an artifact, or really caused by silver atoms that have been deflected in the magnetic field and shown up by my process of development?" "Was that track in the cloud chamber a chance disturbance, or really an evidence of transfer of momentum?" On the basis of many such nightly reflections, that which has objective validity is finally abstracted out from the welter of subjective experience in which scientists as well as other human beings are immersed.—Excerpt from RICHARD C. TOLMAN, "Physical science and philosophy," *Sci. Mo.* **57**, 168 (1943). D.R.

Check List of Periodical Literature

A simple method of demonstrating diffraction grating effects. L. Bragg and H. Lipson, *J. Sci. Inst.* **20**, 110–113 (1943). This apparatus affords observations of Fraunhofer diffraction at very small angles and provides a simple way of demonstrating diffraction by comparatively very large objects. The orders of diffraction from gratings of about 1-mm spacing are well separated, permitting demonstration of effects due to faults in gratings such as periodic error in spacing or in length of lines. Several gratings are described.

The songs of insects. G. W. Pierce, *J. Frank. Inst.* **236**, 141–146 (1943). Another example of recreational physics.

The advent of microscopes in America. F. T. Lewis, *Sci. Mo.* **57**, 249–259 (1943). Includes notes on their early history.

Some notes and suggestions on the teaching of physics. C. J. Smith, *Phil. Mag.* **33**, 775–815 (1942). Faulty or inadequate treatments of the following topics or experiments are discussed: ideal gas equation; variation of pressure of a saturated vapor with surface curvature and temperature; kinetic theory interpretation of gas viscosity; thermal conduction in gases and solids; thermal capacity, including the method of cooling; specific entropy and entropy; bending of beams and cantilevers; measurement of surface tension; variation of viscosity of a liquid with temperature; heat loss from a calorimeter; laws of thermal radiation; optics notation; lenses, prisms and gratings.

The concept of energy. A. E. Bell, *Nature* **151**, 519–523 (1943). On the origin and development of the concept.

Nonsolar planets. A. Hunter, *Nature* **152**, 66–67 (1943). No planet reflecting as little light, relatively speaking, as, say, Jupiter could be seen or photographed even if it accompanied our nearest stellar neighbor. Yet we are not precluded from finding an answer to the long-standing query of whether stars other than our sun have planetary systems; this could now be decided relatively quickly by extending the program of parallax observations somewhat, through international cooperation. A definitive answer obviously would bear importantly on theories of the origin of our solar system.

Antoine Laurent Lavoisier, 1743–1794. J. R. Partington, *Nature* **152**, 207–208 (1943). A biographical sketch.

Edmond Halley and Geomagnetism. S. Chapman, *Nature* **152**, 231–237 (1943). Halley's geomagnetic work in its historical setting; also some of his poems, "a seldom remembered aspect of his remarkable versatility."

Air-age teaching or misinforming. W. J. Luyten, *Science* **97**, 201–202 (1943). A scathing review of the three books comprising the "Air-Age Education Series."

A simple three-color mixer using filtered colors. W. F. Grether, *Science* **98**, 248 (1943). This device for demonstrations does not have the disadvantages inherent in the rotating disk, namely, low saturation, grayish complimentary mixtures and dependence of brightness on room illumination.

Aristotle, Newton, Einstein. E. T. Whittaker, *Science* **98**, 249–254 (1943). Presidential address, Royal Society of Edinburgh, 1942.

Symposium on color blindness. D. Nickerson, D. B. Judd, F. L. Dimmick and E. Murray, *J. Opt. Soc. Am.* **33**, 293–334 (1943).

What is a matrix? C. C. MacDuffee, *Am. Math. Mo.* **50**, 360–365 (1943).

The Heaviside operational calculus. H. B. Curry, *Am. Math. Mo.* **50**, 365–379 (1943). An outline from the algebraic point of view.

An elementary problem. A. Furman, *Am. Math. Mo.* **50**, 386 (1943). A closed tank containing a volume V of water at temperature T_1 has an inlet pipe that supplies water at temperature T_2. If there is no loss of heat and the diffusion in the mixture is instantaneous, show that the temperature of a volume v of water drawn from the tank into an open container is

$$T_2+(T_1-T_2)(1-e^{-v/V})\ V/v.$$

Slide rule applications to algebraic equations. A. R. Weber, *J. Eng. Ed.* **33**, 775–780 (1943).

Research at a Government arsenal in cooperation with universities. L. S. Fletcher, *J. Eng. Ed.* **33**, 781–793 (1943). Types of research sponsored by the Frankford Arsenal.

Author Index to Volume 11

In this index are listed the names of the authors and the titles of their articles. Abstracts of papers read at meetings and of articles that appeared in the "Digest of Periodical Literature" section of the journal are not listed; they are indexed in the Analytic Subject Index.

Analytic Subject Index to Volume 11

The titles of articles are disregarded, the entries being based on analyses of the contents of the original articles. (D) designates digest that has appeared under "Digests of Periodical Literature" or abstract of paper read at a meeting; (T) designates title only; (R) designates review of book or pamphlet.

To facilitate reference to any desired subject, the index is divided into sections arranged alphabetically. The titles of these sections and of cross-references to them are as follows:

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